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VON BAER AND THE RISE OF EMBRYOLOGY.

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THE process of building an animal's body is one of the most wonderful in all nature. After three weeks' incubation of the hen's egg, for illustration, the young bird steps into the world with heart, lungs, brain, eyes and other organs completely formed, which straightway adjust themselves to the new conditions of life. At the beginning of incubation, this living, breathing organism was merely a single element of structure—a cell, or at most a group of a few cells surrounded by a quantity of nourishment in the form of yolk—potentially an animal, in reality simply an egg.

To arrive at the period of hatching, a succession of changes has taken place whereby the food material has been transformed into the living matter of organic units, and these have become aggregated into the tissues of the body. That such a sweeping change has been wrought in such a short time is a marvel of organic architecture involving much more than mere rearrangement of material.

The history of the development of a single individual becomes endowed with greater interest, when observation teaches us that all animals, in the process of becoming, pass through a similar series of steps. In whatever group of living forms the penetrating insight of the scientific observer has been turned, fortified by the microscope, there is the same remarkable story—complex living forms arising from relative simplicity to great complexity in a short time. Every organism, above the very lowest, no matter how complex, starts its existence in the condition of a single microscopic cell, and between that simple state and the fully formed condition every gradation of structure is exhibited. Each time an animal is developed these constructive changes are repeated in orderly sequence.

But, strangely enough, the course of development in any higher organism is not straightforward, but devious. The bird, as well as all higher animals, acquires gill-clefts and other rudimentary structures not adapted to its condition of life. Most of the rudimentary organs are transitory and bear testimony, as hereditary survivals, to the line of ancestry. They are clues by means of which phases in the evolution of animal life may be deciphered.

Bearing in mind these shifting changes, one begins to see why the adult structures of animals are so difficult to understand. They are not only complex they are also greatly modified. The adult condition of any organ or tissue represents the last step in a series of gradually acquired modifications, and is, therefore, the farthest departure from that which is ancestral and archetypal. But in the process of formation all the simpler conditions are exhibited. If, therefore, we wish to understand an organ or an animal we must follow its development, and see it in simpler conditions, before the great modifications have been added.

The tracing of the stages whereby cells merge into tissues, tissues into organs, and how the organs by combinations build up the body, is embryology. It has become one of the richest and most suggestive of the biological sciences in furnishing clues to the past history of animals and throwing light on their relationships.

It is the purpose of this paper to trace in a summary way the rise of that interesting division of biological science, pointing out its epochs, and telling something about the men who laid its foundations, the discussion being limited to the animal side, with no attempt to represent the rise of plant embryology. That we can 'read all history in the lives of a few great men' is essentially true in reference to the progress of embryology. There are many individual workers, each contributing his share, but the ideas of the science are molded into effective form only in the minds of the leaders. In this group of 'the leaders,' Von Baer stands as a monumental figure, at the parting of the ways between the new and the old—the sane thinker, the great observer.

The story of the rise of embryology can, for convenience, be divided into five periods each marked by an advance in general knowledge. These are: (1) the period of Harvey and Malpighi; (2) the period of Wolff; (3) the period of Von Baer; (4) the period from Von Baer to Balfour; and (5) the period of Balfour with an indication of present tendencies.

The Period of Harvey and Malpighi.

In General.—The conventional way of looking at the rise of embryology has been derived mainly through the channels of German scholarship. But there is reason to depart from the traditional aspect

of the subject, in which Wolff is heralded as its founder, and the one central figure prior to Pander and Von Baer.

The embryological work of Wolff's great predecessors Harvey and Malpighi has been passed over too lightly. Although these men have received ample recognition in closely related fields of investigation, their insight into those mysterious events which culminate in the formation of a new animal has been rarely appreciated. Now and then a few writers, as Brooks and Whitman, have pointed out the great worth of Harvey's work in embryology, but fewer have spoken for Malpighi in this connection. Koelliker, it is true, in his address at the unveiling of the statue of Malpighi, in his native town of Crevalcuore, in 1894, gives him well-merited recognition as the founder of embryology, and Sir Michael Foster has written in a similar vein in his delightful 'Lectures on the History of Physiology.'

However great was Harvey's work in embryology, I venture to say that Malpighi's was greater when considered as a piece of observation. Harvey's work is more philosophical; he discusses the nature of development and shows unusual powers as an accurate reasoner. But that part of his treatise devoted to observation is far less extensive and exact than Malpighi's, and throughout his lengthy discussions he has the flavor of the ancients.

Malpighi's work, on the other hand, flavors more of the moderns. In terse descriptions, and with many sketches, he shows the changes in the hen's egg from the close of the first day of development onwards.

It is a noteworthy fact that, at the period in which he lived, Malpighi could so successfully curb the tendency to indulge in wordy disquisitions, and that he was satisfied to observe carefully, and tell his story in a simple way. This quality of mind can not be too much admired. As Emerson has said: "I am impressed with the fact that the greatest thing a human soul ever does in this world is to see something and tell what it saw in a plain way. Hundreds of people can talk for one who can think, but thousands can think for one who can see. To see clearly is poetry, philosophy and religion all in one." But 'to see' here means to interpret as well as to observe. Harvey was also an original observer, but, in embryology, not in so eminent a degree as Malpighi.* Could we have had the insight of Harvey united to the observing powers of Malpighi, we should have had an almost perfect combination.

Although there were observers in the field of embryology before

* Notwithstanding the deserved praise of Malpighi as an observer, it may be remarked, in passing, that he was not the leader of his period in pure observation and description. Swammerdam showed even greater powers for critical and finished work in this direction. (See 'Malpighi, Swammerdam, and Leeuwenhoek,' POP. SCI. MO., April, 1901.

Harvey, little of substantial value had been produced. The earliest attempts were vague and uncritical, and, naturally, embraced only fragmentary views of the more obvious features of body formation. Nor, indeed, are we to look for much advance in the field of embryology even in Harvey's time. The reason for this will be obvious when we remember that the renewal of independent observation had just been brought about in the preceding century, when, in 1543, the science of anatomy had been reformed by Vesalius.

Harvey himself was one of the pioneers in the intellectual awakening. By his immortal discovery of circulation of the blood (1619–1628) he had established a new physiology. Now, studies on the development of the body are more special, they involve observations on minute structures and recondite processes, and must, therefore, depend upon considerable advances in anatomy and physiology. Naturally the science of embryology came later.

Harvey.—Harvey's was the first attempt to make a critical analysis of the process of development, and that he did not attain more was not due to limitations of his powers of discernment, but to the necessity of building on the general level of the science of his time, and, further, to his lack of instruments of observation and technique. Nevertheless, Harvey may be considered as having made the first independent advance in embryology.

By clearly teaching, on the basis of his own observations, the gradual formation of the body by aggregation of its parts, he anticipated Wolff. This doctrine came to be known under the title of 'epigenesis,' but Harvey's epigenesis* was not, as Wolff's was, directed against a theory of predelineation of the parts of the embryo, but against the ideas of the medical men of the time regarding the metamorphosis of germinal elements. It lacked, therefore, the dramatic setting which surrounded the work of Wolff in the next century. Had the doctrine of preformation been current in Harvey's time, we are quite justified in assuming that he would have assailed it as vigorously as Wolff did.

Harvey's embryological work was published in 1651 under the title 'Exercitationes de Generatione Animalium.' It embraces not only observations on the development of the chick, but also on the deer and some other mammals. He being the court physician of Charles I., that sovereign had many deer killed in the park, at intervals, in order to give Harvey the opportunity to study their development.

As fruits of his observation on the chick, he showed the position† in which the embryo arises within the egg, viz., in the white opaque

* As Whitman has pointed out, Aristotle taught epigenesis as clearly as Harvey and is, therefore, to be regarded as the founder of that conception.

† Fabricius supposed that the chick developed from the twisted cords of the white of the egg.

spot or cicatricula. He also corrected Aristotle, Fabricius and his other predecessors in many particulars.

Harvey's greatest predecessor in this field, Fabricius, was also his teacher. When, in search of the best training in medicine, Harvey wended his way from England to Italy, in Padua, he came under Fabricius as one of his teachers. In 1600, Fabricius published the earliest illustrations on the development of animals, and, again, in 1625, six years after his death, appeared his illustrated treatise on the development of the chick.† Altogether his figures show developmental stages of the cow, sheep, pig, galeus, serpent, rat and chick. The value of his work may be easily overestimated by a casual examination of the plates.

Harvey's own treatise was not illustrated. With that singular independence of mind which he showed in all his work, the vision of the pupil was not hampered by the authority of his teacher, and, trusting only to his own sure observation and reason, he described the stages of development as he saw them in the egg, and placed his own construction on the facts.

One of the earliest things to arrest his attention in the chick was a pulsating point, the heart, and, from this observation, he supposed that the heart and blood were the first formations. He says: "But as soon as the egg, under the influence of the gentle warmth of the incubating hen, or of warmth derived from another source, begins to pullulate, this spot forthwith dilates, and expands like the pupil of the eye, and from thence, as the grand center of the egg, the latent plastic force breaks forth and germinates. This first commencement of the chick, however, so far as I am aware, has not yet been observed by any one."

It is to be understood, however, that his descriptive part is relatively brief (about 40 pages out of 350 in Willis's translation), and that the bulk of the 106 'exercises' into which his work is divided is devoted to comments on the older writers and discussions of the nature of the process of development.

Portraits of Harvey are by no means uncommon. The one in the National Portrait Gallery, in London, is represented in Fig. 1. This is usually regarded as the second-best portrait of Harvey, since the one painted by Jansen, now in possession of the Royal College of Physicians, is believed to be the best one extant. Permission to reproduce the latter is not given.

The picture in the National Gallery shows a countenance of composed intellectual strength, with a suggestion, in the forehead and outline of the face, of some of the portraits of Shakespeare.

The aphorism '*Omne vivum ex ovo*,' though not invented by

† The earliest figures on the development of the chick are probably those of Coiter, 1573.

Harvey, was brought into general use through his works. It is not to be taken in its full modern significance. With Harvey it meant simply that the embryos of all animals, the viviparous as well as the oviparous, originate in eggs, and it was directed against certain contrary medical theories of the time.

The first edition of his 'Generatione Animalium,' London, 1651, is provided with an allegorical title-page embodying this idea. As

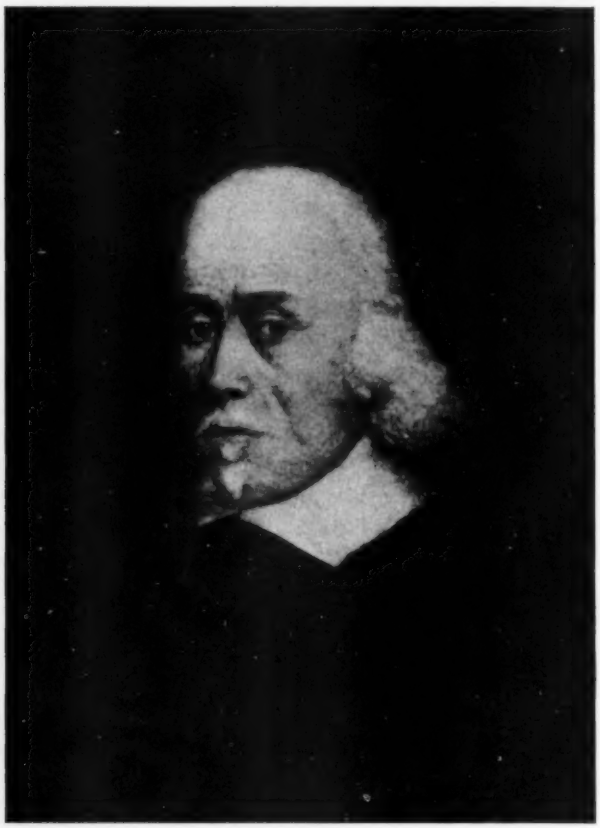

FIG 1. WILLIAM HARVEY (1578-1657).

shown in Fig. 2, it represents Jove on a pedestal, uncovering a round box—or ovum—bearing the inscription '*ex ovo omnia*' and from the box issue all forms of living creatures including also man.

Malpighi.—The observer in embryology who looms into prominence between Harvey and Wolff, is Malpighi. He supplied what was greatly needed at the time—an illustrated account of the actual stages in development of the chick from the end of the first day to hatching, shorn of verbose references and speculations.

His observations on development are in two separate memoirs, both

FIG. 2. TITLE-PAGE TO HARVEY'S *Generatione Animaliun* (1651).

sent to the Royal Society in 1672, and published by the society in Latin, under the titles 'De Formatione Pulli in Ovo' and 'De Ovo Incubato.' The two taken together, are illustrated by twelve plates containing eighty-six figures, and the twenty--two quarto pages of text are nearly all devoted to descriptions.

His pictures, although not correct in all particulars, represent what he was able to see, and are very remarkable for the age in which they were made, and considering the instruments of observation at his

command. They show successive stages from the time the embryo is first outlined, and, taken in their entirety, they cover a wide range of stages.

His observations on the development of the heart, comprising twenty figures, are the most complete. He clearly illustrates the aortic arches,—those transitory structures of such great interest as showing a phase in ancestral history.

He was also the first to show by pictures the formation of the head-fold and neural groove as well as the brain vesicles and eye

FIG. 3. MARCELLUS MALPIGHI (1628–1694.)

pockets. His delineation of heart, brain and eye vesicles are far ahead of those illustrating Wolff's 'Theoria Generationis' made nearly a hundred years later. But Wolff rose to a higher level in his later work on the development of the intestine, and produced some figures better than any of Malpighi's.

The original drawings for 'De Ovo Incubato,' still in possession of the Royal Society, are made in pencil and red chalk. They far surpass the reproductions in finish and accuracy. In looking them over in 1902, I noticed four aortic arches represented in one figure where the engraver has shown only three.

The portrait of Malpighi shown in Fig. 3 is taken from his Life

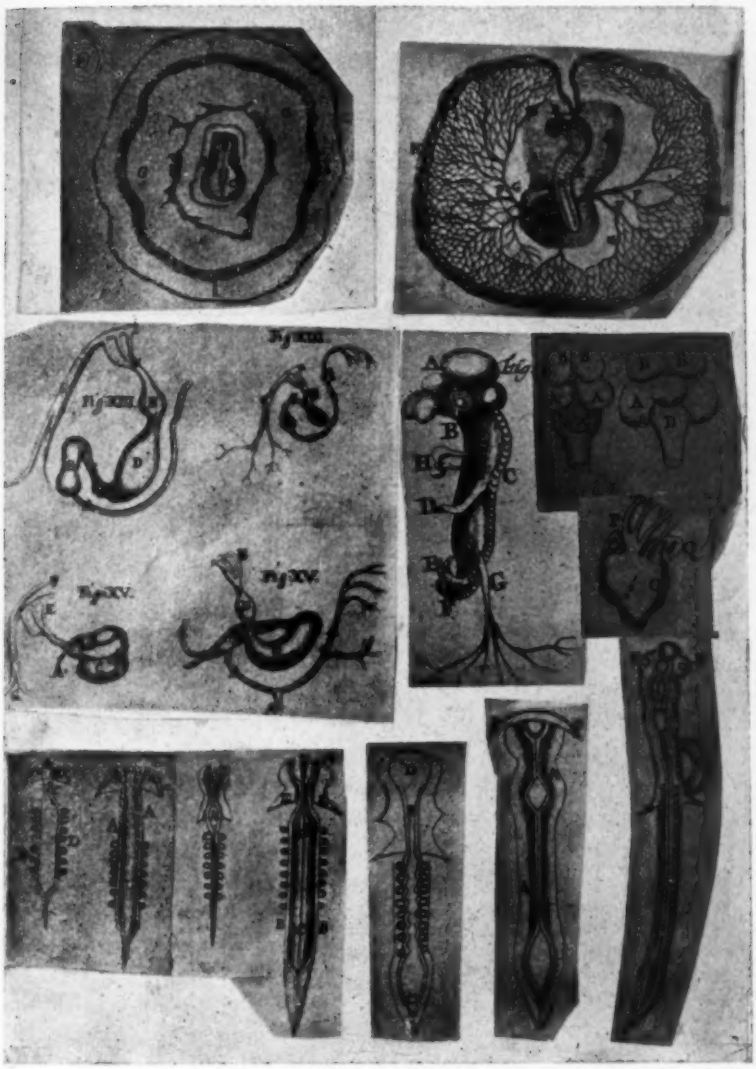

FIG 4. SELECTED SKETCHES FROM MALPIGHI'S WORKS SHOWING STAGES IN THE DEVELOPMENT OF THE CHICK (1672).

by Atti. From descriptions of his personal appearance, it is probably a better likeness than the handsome idealized portrait painted by Tabor, and presented by Malpighi to the Royal Society of London.*

Fig. 4 shows a few selected figures from the various plates of his embryological treatises, to compare with those of Wolff.

While Harvey taught the gradual formation of parts, Malpighi, from his own observations, supposed the rudiments of the embryo to

* For a reproduction of that portrait see POP. SCI. MO., Vol. LVIII., 1901, p. 563.

preexist in the egg. He thought that, possibly, the blood vessels were in the form of tubes, closely wrapped together, which by becoming filled with blood were distended. Nevertheless, in the treatises mentioned above he is very temperate in his expressions on the whole matter, and evidently believed in the new formation of many parts. In the work published after his death he appears to have been less circumspect.

Malpighi's work, with that of some of his contemporaries, marks the beginning of the theory of preformation.*

On the whole, Malpighi should rank above Harvey as an embryologist, on account of his discoveries and fuller representation, by drawings and descriptions, of the process of development. As Sir Michael Foster has said: "The first adequate description of the long series of changes, by which, as they melt the one into the other, like dissolving views, the little white opaque spot in the egg is transformed into the feathered, living, active bird, was given by Malpighi. And where he left it, so for the most part the matter remained until even the present century. For this reason we may speak of him as the founder of embryology."

The Period of Wolff.

Between Harvey and Wolff, embryology had become dominated by the theory that the embryo exists already preformed within the egg, and, as a result of the rise of this new doctrine, the publications of Wolff had a different setting from that of any of his predecessors. It is only fair to say that to this circumstance is owing, in large part, the prominence of his name in connection with the theory of epigenesis. As we have already seen, Harvey, more than a century before the publications of Wolff, had clearly taught that development was a process of gradual becoming. Nevertheless, Wolff's work as opposed to the new theory was very important.

While the facts fail to support the contention that he was the founder of epigenesis, it is to be remembered that he has claims in other directions to rank as the foremost student of embryology prior to Von Baer.

As a preliminary to discussing Wolff's position we should bring under consideration the doctrine of preformation and encasement.

Rise of the Theory of Predelineation.—The idea of preformation in its first form is easily set forth. Just as when we examine a seed, we find within an embryo plantlet, so it was supposed that the various forms of animal life existed in miniature within the egg. The process of development was supposed to consist of the expansion or unfolding of this preformed embryo. The process was commonly illustrated by reference to flower buds. "Just as already in a small bud all the

* See further under the period of Wolff.

parts of the flower, such as stamens and colored petals, are enveloped by the green and still undeveloped sepals,—just as the parts grow in concealment and then suddenly expand into a blossom, so also in the development of animals it was thought that the already present small but transparent parts grow, gradually expand, and become discernible."* From the feature of unfolding this was called in the eighteenth century the theory of *evolution,* giving to that term quite a different meaning from that accepted at the present time.

This theory, strange as it may seem to us now, was founded on a basis of actual observation—not entirely on speculation. Although it was a product of the seventeenth century, from several printed accounts one is likely to gather the impression that it arose in the eighteenth century and that Bonnet, Haller and Leibnitz were among its founders. This implication is in part fostered by the circumstance that Swammerdam's 'Biblia Naturæ,' which contains the germ of the theory, was not published until 1737—more than a half century after his death—although the observations for it were completed before Malpighi's first paper on embryology was published in 1672. While it is well to bear in mind that date of publication, rather than date of observation, is accepted as establishing the period of emergence of ideas, there were other men, such as Malpighi and Leeuwenhoek, contemporaries of Swammerdam, who published in the seventeenth century the basis for this theory.

Malpighi supposed (1672) the rudiment of the embryo to preexist within the hen's egg, because he observed evidences of organization in the unincubated egg. This was in the heat of the Italian summer (in July and August, as he himself records), and Dareste suggests that the developmental changes had gone forward to a considerable degree before Malpighi opened the eggs. Be this as it may, the imperfection of his instruments and technique would have made it very difficult to have seen anything definitely in stages under twenty-four hours.

In reference to his observations he says that, in the unincubated egg, he saw a small embryo enclosed in a sac which he subjected to the rays of the sun. "Frequently I opened the sac with the point of a needle so that the animals contained within might be brought to the light, nevertheless to no purpose: for the individuals were so jelly-like and so very small that they were lacerated by a light stroke. Therefore it is right to confess that the beginnings of the chick preexist in the egg and have reached a higher development in no other way than in the eggs of plants." ("Quare *pulli stamina* in ovo *præexistere,* altiorémque originem nacta esse fateri convenit, haud dispari ritu, ac in Plantarum ovis.")

* O. Hertwig.

Swammerdam (1637–1680) supplied a somewhat better basis. He observed that the parts of the butterfly, and other insects as well, are discernible in the chrysalis stage. Also, on observing caterpillars just before going into the pupa condition, he saw in outline the organs of the future stage, and very naturally concluded that development consists of an expansion of already formed parts.

A new feature was introduced through the discovery, by Leeuwenhoek about 1677,* of the fertilizing filaments of eggs. Soon after, controversies began to arise as to whether the embryo preexisted in the sperm or in the egg. By Leeuwenhoek, Hartsoeker and others the egg was looked upon as simply a *nidus* within which the sperm developed, and they asserted that the future animal existed in miniature in the sperm. These controversies gave rise to the schools of the Animalculists, who believed the sperm to be the animal germ, and of the Ovists, who contended for the ovum in that rôle.

One of the curiosities of this period is shown in Fig. 5, taken from an old Dutch edition of Leeuwenhoek's works, in which he undertakes to represent predelineation of both sexes within the sperm.

FIG. 5. SKETCHES ILLUSTRATING PRE-DELINEATION OF THE EMBRYO WITHIN THE SPERM. FROM AN OLD EDITION OF LEEUWENHOEK'S WORKS.

It is interesting to follow the metaphysical speculations which led to another aspect of the doctrine of preformation. There were those, notably Swammerdam, Leibnitz and Bonnet, who did not hesitate to follow the idea to the logical consequence, that, if the animal germ exists preformed, one generation after another must be encased within it. This gave rise to the fanciful idea of encasement or *emboîtement* which was so greatly elaborated by Bonnet and, by Leibnitz, applied to the development of the soul. Even Swammerdam (who, by the way, although a masterly observer, was always a poor generalizer) conceived the mental picture of the germs of all forthcoming generations having been located in the common mother Eve, all closely encased one within the other, like the boxes of a Japanese juggler. The end of the human race was conceived of by him as a necessity, when the last germ of this wonderful series had been unfolded.

His successors, in efforts to compute the number of homunculi,

* The discovery is also attributed to Hamm, a medical student, and to Hartsoeker, who claimed priority in the discovery.

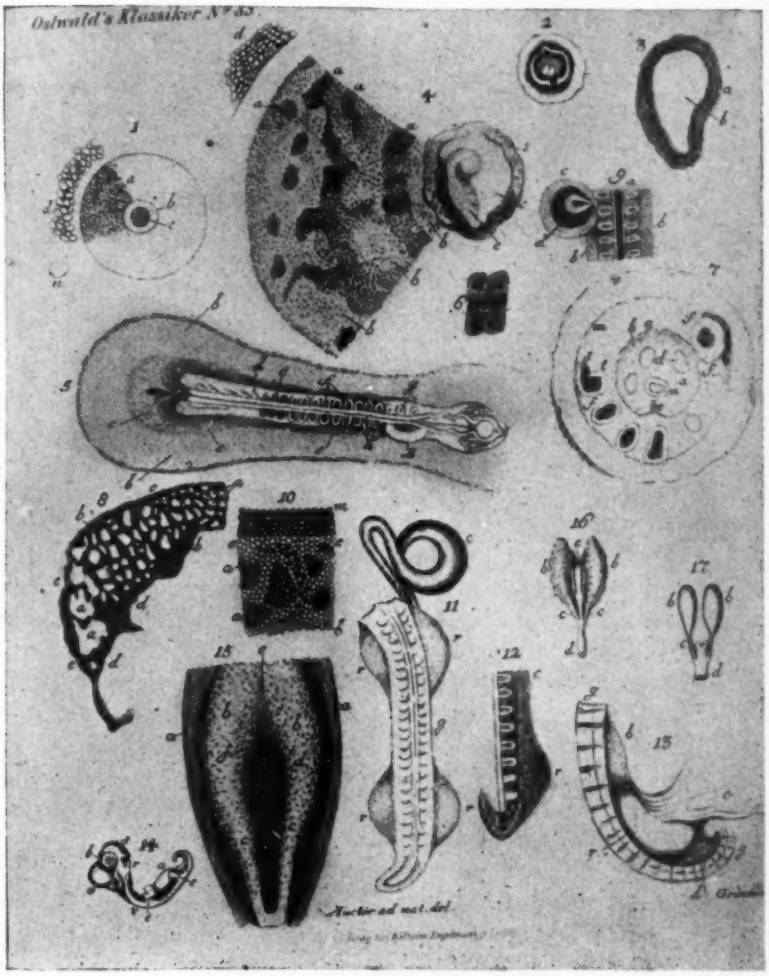

FIG. 6. PLATE FROM WOLF'S *Theoria Generationis* (1759), SHOWING STAGES IN THE DEVELOPMENT OF THE CHICK.

which must have been condensed in the ovary of Eve, arrived at the amazing result of two hundred millions.

Work of Wolff.—Wolff, as a young man of 26 years, set himself against this grotesque doctrine of preformation and encasement, in his 'Theoria Generationis,' published in 1759. This consists of three parts: One devoted to the development of plants, one to the development of animals and one to theoretical considerations. He contended that the organs of animals make their appearance gradually, and that he could actually follow their successive stages of formation.

The figures in it illustrating the develoment of the chick, some of which are shown in Fig. 6, are not, on the whole, so good as Malpighi's. Wolff gives in all seventeen figures, while Malpighi published eighty-

six, and his twenty figures on the development of the heart are more detailed than any of Wolff's. When the figures represent similar stages of development, a comparison of the two men's work is favorable to Malpighi. The latter shows much better, in corresponding stages, the series of cerebral vesicles and their relation to the optic vesicles. Moreover, in the wider range of his work, he shows many things—such as the formation of the neural groove, etc.—not included in Wolff's observations. Wolff, on the other hand, figures for the first time the primitive kidneys, or 'Wolffian bodies,' of which he was the discoverer.

Although Wolff was able to show that development consists of a gradual formation of parts, his theory of development was entirely mystical and unsatisfactory. The fruitful idea of germinal continuity had not yet emerged, and the thought that the egg has inherited an organization from the past was yet to be expressed. Wolff was therefore in the same quandary as his predecessors when he undertook to explain development. Since he assumed a total lack of organization in the beginning, he was obliged to make development 'miraculous' through the action on the egg of a hyperphysical agent. From a total lack of organization, he conceived of its being lifted to the highly organized product, through the action of a *'vis essentialis corporis.'*

He returned to the problem of development later, and, in 1768–69, published his best work in this field on the development of the intestine.* This is a very original and strong piece of observational work. While his observations for the 'Theoria Generationis' did not reach the level of Malpighi's those of the paper of 1768 surpassed it and held the position of the best piece of embryological work up to that of Pander and Von Baer. This work was so highly appreciated by Von Baer that he said: 'It is the greatest masterpiece of scientific observation which we possess.' In it he clearly demonstrated that the development of the intestine, and its appendages, is a true process of becoming. Still later, in 1789, he published further theoretical considerations.

But all Wolff's work was launched into an uncongenial atmosphere. The great physiologist, Haller, could not accept the idea of epigenesis, but opposed it energetically, and, so great was his authority, that the ideas of Wolff gained no currency. This retarded progress in the science of animal development for more than a half century.

In 1812, the elder Meckel, recognizing the great value of Wolff's researches on the development of the intestine, rescued the work from neglect and obscurity, by publishing a German translation of the same, and bringing it to the attention of scholars. From that time onward Wolff's work began to be fruitful.

* 'De Formatione Intestinorum,' *Nova Commentar, Ac. Sci. Petrop.*, St. Petersburg, XII., 1768; XIII., 1769.

His 'Formatione Intestinorum' embodies his greatest contribution to embryology rather than his 'Theoria Generationis'; not only is it a more fitting model of observation, but in it he foreshadows the idea of germ-layers in the embryo, which, under Pander and Von Baer, became the fundamental conception in structural embryology. Throughout his work, both early and late, he likens the embryonic rudiments, which precede the formation of organs, to leaflets. In his work of 1768, he describes in detail how the leaf-like layers give rise to the systems of organs: Showing that the nervous system arises first from a leaf-like layer, and is followed, successively, by a flesh-layer, the vascular system and, lastly, by the intestinal canal—all arising from original leaf-like layers.

In these important generalizations, although they are verbally incorrect, he reached the truth as nearly as it was possible at the time, and laid the foundation of the germ-layer theory.

Wolff was a man of great power as an observer, and although his influence was for a long time retarded, he should be recognized as the foremost investigator in embryology before Von Baer.

The little known of his life is gained through his correspondence and a letter by his amanuensis. Through personal neglect, and hostility to his work, he could not secure a foothold in the universities of Germany, and, in 1764, on the invitation of Catharine of Russia, he went to the Academy of Sciences at St. Petersburg, where he spent the last thirty years of his life.

His sincere and generous spirit is shown in his correspondence with Haller, his great opponent. "And as to the matter of contention between us, I think thus: For me, no more than for you glorious man, is truth of the very greatest concern. Whether it chance that organic bodies emerge from an invisible into a visible condition, or form themselves out of the air, there is no reason why I should wish the one were truer that the other, or wish the one and not the other. And this is your view also, glorious man. We are investigating for truth only: we seek that which is true. Why then should I contend with you?"

I have not been able to locate a portrait of Wolff, although I have sought one in various ways for several years. The Secretary of the Academy of Sciences at St. Petersburg writes that no portrait of Wolff exists there, and that they will gratefully receive information regarding any existing portrait of the great academician.

The Period of Von Baer.

What Verworn says of Johannes Müller's position in physiology, may with equal appropriateness be applied to Von Baer in the science of embryology. He was: "One of those monumental figures that the history of every science brings forth but once. They change the whole

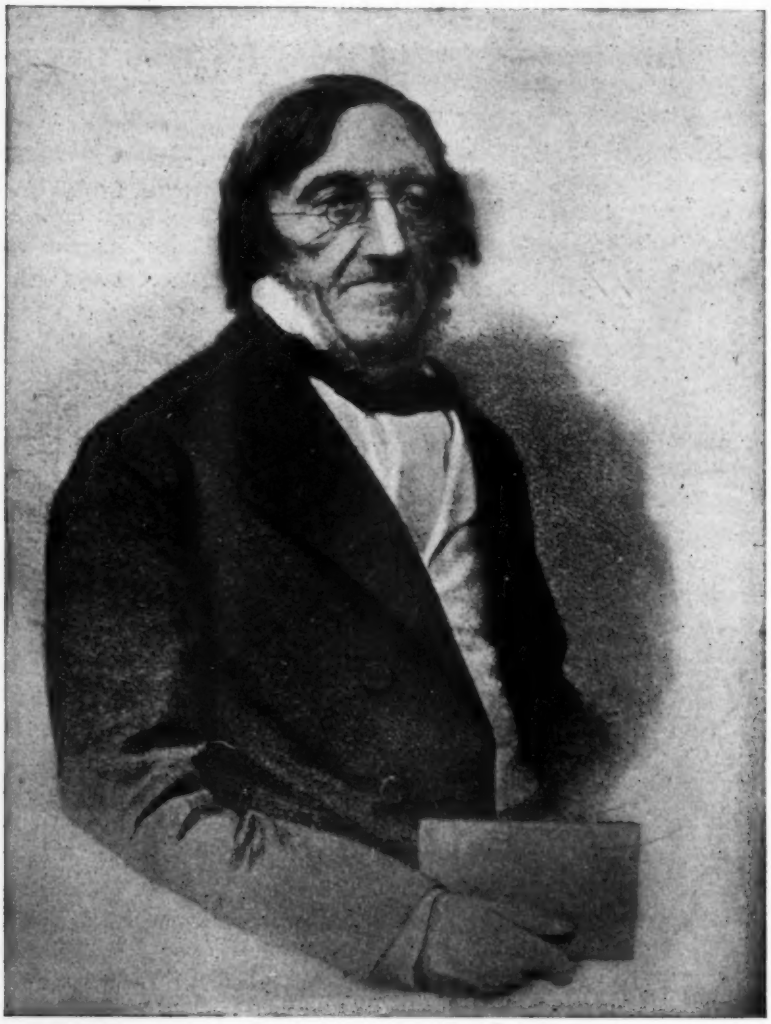

FIG. 7. K. ERNST VON BAER AT ABOUT SEVENTY YEARS OF AGE.

aspect of the field in which they work and all later growth is influenced by their labors."

The greatest classic in embryology is his 'Entwickelungsgeschichte der Tiere—Beobachtung und Reflexion,' the first part of which was published in 1828, and the work on the second part completed in 1834, although it was not published till 1837. This second part was never finished according to the plan of Von Baer, but was issued by his publisher, after vainly waiting for the finished manuscript. The final portion, which Von Baer had withheld, in order to perfect in some particulars, was published in 1888, after his death, but in the form in which he had left it in 1834.

The observations for the first part began in 1819, after he had received a copy of Pander's researches and covered a period of seven years of close devotion to the subject, and the observations for the last part were carried on at intervals for several years.

It is significant of the character of his 'Reflexionen' that, although published before the announcement of the cell-theory, and before the acceptance of the doctrine of organic evolution, they have exerted a moulding influence upon embryology to the present time. The position of Von Baer in embryology, is due as much to his sagacity in speculation, as to his powers as an observer. "Never again have observation and thought been so successfully combined in embryological work" (Minot).

Von Baer was born in 1792, and lived on to 1876, but his enduring fame in embryology rests on work completed more than forty years before the end of his useful life. After his removal from Königsberg to St. Petersburg, in 1834, he very largely devoted himself to anthropology in its widest sense, and thereby extended his scientific reputation into other fields.

If space permitted, it would be interesting to give the biography* of this extraordinary man, but here, it will be necessary to content ourselves with an examination of his portrait and a brief account of his work.

Several portraits of Von Baer showing him at different periods of his life have been published. A very attractive one, taken in his early manhood, appeared in *Harper's Magazine* for 1898. The expression of the face is poetical, and the picture is interesting to compare with the more matured sage-like countenance forming the frontispiece of Stieda's 'Life of Von Baer.' This, perhaps best of all his portraits, shows him in the full development of his powers. An examination of it impresses one with confidence in his balanced judgment and the thoroughness and profundity of his mental operations.

The portrait of Von Baer at about seventy years of age, reproduced in Fig. 7 is destined to be the one by which he is commonly known to embryologists, since it forms the frontispiece of the great cooperative 'Handbook of Embryology' now appearing under the editorship of Oskar Hertwig.

Apart from special discoveries, Von Baer greatly enriched embryology in three directions: In the first place, he set a higher standard for all work in embryology and thereby lifted the entire science to a higher level. Activity in a great field of this kind is, with the rank and file of workers, so largely imitative that this feature of his influence

* Besides biographical sketches by Stieda, Waldeyer and others, we have a very entertaining autobiography of Von Baer, published in 1864, for private circulation, but afterwards (1866) reprinted and placed on sale.

should not be overlooked. In the second place, he established the germ-layer theory, and, in the third, he made embryology comparative.

In reference to the germ-layer theory, it should be recalled that Wolff had distinctly foreshadowed the idea, by showing that the material out of which the embryo is constructed is, in an early stage of development, arranged in the form of leaf-like layers. He showed specifically that the alimentary canal is produced by one of these sheet-like expansions folding and rolling together.

Pander, by observations on the chick (1817), had extended the knowledge of these layers and elaborated the conception of Wolff. He recognized the presence of three primary layers, an outer, a middle and an inner, out of which the tissues of the body are formed.

But, it remained for Von Baer,* by extending his observations into all the principal groups of animals, to raise this conception to the rank of a general law of development. He was able to show that in all animals except the very lowest, there arises in the course of development leaf-like layers, which become converted into the 'fundamental organs' of the body.

Now, these elementary layers are not definitive tissues of the body, but are embryonic, and therefore, may appropriately be designated 'germ-layers.' The conception that these germ-layers are essentially similar in origin and fate, in all animals, was a fuller and later development of the germ-layer theory, which dominated embryological study until a recent date.

Von Baer recognized four such layers: the outer and inner ones being formed first, and, subsequently budding off a middle layer composed of two sheets. A little later (1845) Remak recognized the double middle layer of Von Baer as a unit, and thus arrived at the fundamental conception of three layers—the ecto-, endo- and mesoderm—which has so long held sway. For a long time after Von Baer, the aim of embryologists was to trace the history of these germ-layers —and so in a wider and much qualified sense it is to-day.

It will ever stand to his credit, as a great achievement, that Von Baer was able to make a very complicated feature of development clear

* It is of more than passing interest to remember that Pander and Von Baer were associated as friends and fellow students, under Döllinger at Würzburg. It was partly through the influence of Von Baer that Pander came to study with Döllinger, and took up investigations on development. His ample private means made it possible for him to bear the expenses connected with the investigation, and to secure the services of a fine artist for making the illustrations. The result was a magnificently illustrated treatise. His unillustrated thesis in Latin (1817) is more commonly known, but the illustrated treatise in German is rarer. Von Baer did not take up his researches seriously until Pander's were published. It is significant of their continued harmonious relations that Von Baer's work is dedicated 'An meinen jugendfreund, Dr. Christian Pander.'

and relatively simple. Given a leaf-like rudiment, with the layers held out by the yolk, as is the case in the hen's egg, and it was no easy matter to conceive of how they are transformed into the nervous system, the body wall, the alimentary canal and other parts, but, Von Baer saw deeply and clearly that the fundamental anatomical features of the body are assumed by the leaf-like rudiments being rolled into tubes.

Fig. 8 shows four sketches taken from the plates illustrating Von Baer's work. At *A* is shown a stage in the formation of the embryonic

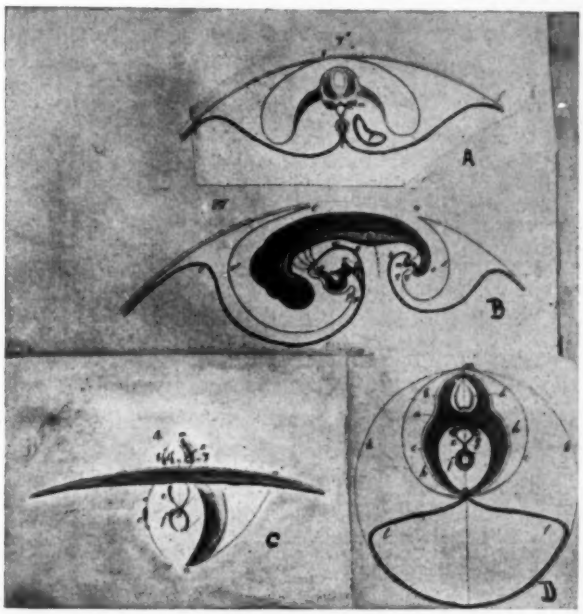

FIG. 8. SKETCHES FROM VON BAER'S EMBRYOLOGICAL TREATISE (1828).

envelope, or amnion, which surrounds the embryos of all animals above the class of amphibia. At *B*, another figure of an ideal section, shows that long before the day of microtomes, Von Baer made use of sections to represent the relationships of his four germ-layers. At *C* and *D* is represented, diagramatically, the way in which these layers are rolled into tubes. He showed that the central nervous system arose in the form of a tube, from the outer layer, the body-wall in the form of a tube, composed of skin and muscle layers, and the alimentary tube from mucous and vascular layers.

The generalization that embryos in development tend to recapitulate their ancestral history is frequently attributed to Von Baer, but the qualified way in which he suggests something of the sort will not justify one in attaching this conclusion to his work.

Von Baer was the first to make embryology truly comparative, and to point out its great value in anatomy and zoology. By embryological

studies, he recognized four types of organization—as Cuvier had done from the standpoint of comparative anatomy. But, since these types of organization have been greatly changed and sub-divided, the importance of the distinction has faded away. But as a distinct break with the old idea of a linear scale of being it was of moment.

Among his especially noteworthy discoveries may be mentioned that of the egg of the human being and other mammals, and the notochord as occurring in all vertebrate animals.

Von Baer has come to be dignified with the title of the 'Father of modern embryology.' No man could have done more in his period, and it is owing to his superb intellect, and talents as an observer, that he accomplished what he did. As Minot says: He 'worked out, almost as fully as was possible at this time, the genesis of all the principal organs from the germ-layers, instinctively getting at the truth as only a great genius could have done.'

After his masterly work the science of embryology could never return to its former level; he had given it a new direction, and through his influence a period of great activity was inaugurated.

The Period from Von Baer to Balfour.

In the period between Von Baer and Balfour there were great general advances in the knowledge of organic structure which brought the whole process of development into a new light.

Among the most important advances are to be enumerated: the announcement of the cell theory, the discovery of protoplasm, the beginning of the recognition of germinal continuity and the establishment of the doctrine of organic evolution.

The Cell Theory.—The generalization that the tissues of all animals and plants are structurally composed of similar units—called cells—was given to the world through the combined labors of Schleiden and Schwann. Schleiden, the botanist, in 1838, and Schwann, the anatomist, in the following year, published the observations on which this truth rests. The investigations stimulated by the announcement of this theory soon resulted in showing that the conception of the cell entertained by the founders was very imperfect, and, by 1860, the original theory had been molded into the protoplasm doctrine of Max Schultze.

The modification of the cell theory did not, however, affect the original conception that the cell is a unit of organic structure, but showed that the unit is, essentially, a globule of protoplasm containing a nucleus, and not simply a box-like compartment as Schleiden and Schwann had suggested.

The broad-reaching effects of the cell-theory may be easily imagined since it united all animals on the broad plane of similitude in microscopic structure. Now, for the first time, the tissues of the body were

analyzed into their units; now, for the first time, was comprehended the nature of the germ-layers of Von Baer.

Among the first questions to emerge in the light of the new researches were: What is the origin of the cells in the organs, the tissues and the germ-layers? The road to the investigation of these questions was already opened, and it was followed, step by step, until the egg and sperm came to be recognized as modified cells. This position was reached, for the egg, about 1861, when Gegenbaur showed that the eggs of all vertebrated animals, regardless of size and condition, are in reality single cells. The sperm was put in the same category about 1865.

The rest was relatively easy—the egg, a single cell—by successive divisions produces many cells, and the arrangement of these into primary embryonic layers brings us to the starting point of Wolff and Von Baer. The cells, continuing to multiply by division, not only increase in number, but also undergo changes through division of physiological labor, whereby certain groups are set apart to perform a particular part of the work of the body. In this way arise the various tissues of the body—which are, in reality, similar cells performing a similar function. Finally, from combinations of tissues the organs are formed.

But the egg, before entering on the process of development, must be stimulated by the union of the sperm with the nucleus of the egg, and, thus, the starting point of every animal and plant, above the lowest group, proves to be a single cell with protoplasm derived from two parents. While questions regarding the origin of cells in the body were being answered, the foundation for the embryological study of heredity was also laid.

Advances were now more rapid and more sure, flashes of morphological insight began to illuminate the way, and the facts of isolated observations began to fit into a harmonized whole.

Apart from the general advances of this period, mentioned in other connections, the work of a few individuals requires notice.

Rathke and Remak were engaged with the broader aspects of embryology as well as with special investigations. To Rathke is owing great advances in the knowledge of the development of insects and other invertebrates, and Remak is notable for similar work with the vertebrates. As already mentioned, he was the first to recognize the middle layer as a unit—through which the three germ-layers of later embryologists emerged into the literature.

Koelliker, the veteran embryologist, still living in Würzburg, carried on investigations on the segmentation of the egg. Besides work on the invertebrates, later, he followed with care the development of the chick and the rabbit—he encompassed the whole field of embryology—and published, in 1861 and later, in 1876, a general treatise on

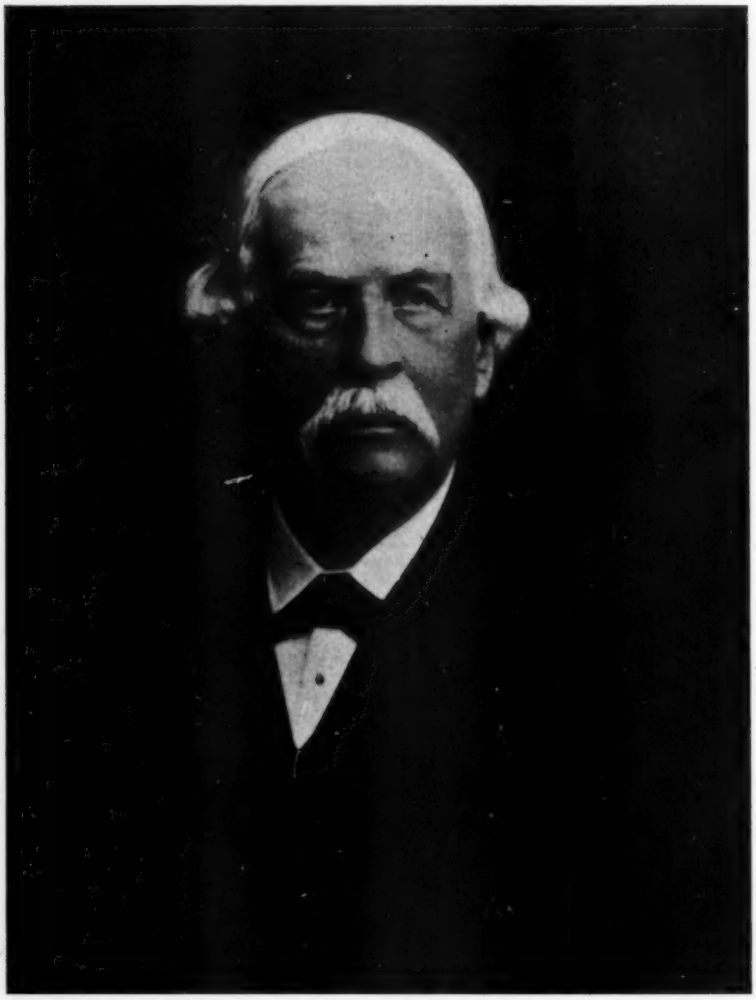

FIG. 9 ALBRECHT KOELLIKER. BORN 1817.

vertebrate embryology of high merit. His portrait is shown in Fig. 9.

Huxley took a great step towards unifying the idea of germ-layers throughout the animal kingdom, when he maintained, in 1849, that the two cell-layers in animals like the hydra, and oceanic hydrazoa, correspond to the ectoderm and endoderm of higher animals.

Kowalevsky, whose portrait is shown in Fig. 10, made interesting discoveries of a general bearing. In 1866 he showed the practical identity, in the early stages of development, between one of the lowest vertebrates (*Amphioxus*) and a tunicate. The latter had up to that time been considered an invertebrate, and the effect of

Kowalevsky's work was to break down the sharply limited line, supposed to exist between the invertebrates and the vertebrates. This was of great influence in subsequent work. Kowalevsky also founded the generalization that all animals in development pass through a gastrula stage,—a doctrine associated, since 1874, with the name of Haeckel under the title of the gastræa theory.

Beginning of the Idea of Germinal Continuity.—The conception that there is unbroken continuity of germinal substance between all living organisms, and that the egg and sperm are endowed with an inherited organization of great complexity, has become the basis for all current theories of heredity and development. So much is involved in this conception, that, in the present decade, it has been designated (Whitman) 'the central fact of modern biology.' The first clear expression of it is found in Virchow's 'Cellular Pathology' published in 1858. It was not, however, until the period of Balfour, and through the work of Fol, Van Beneden (chromosomes, 1883), Boveri, Hertwig and others, that the great importance of the fact began to be appreciated, and the conception began to be woven into the fundamental ideas of development.

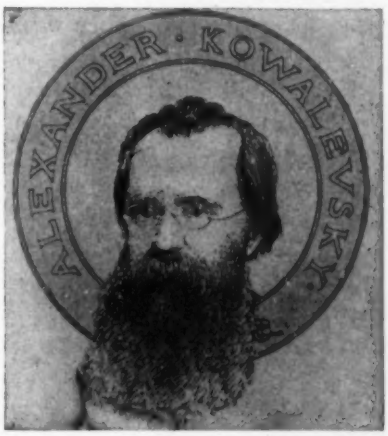

FIG. 10. A. KOWALEVSKY 1840-1901.

Influence of the Doctrine of Organic Evolution.—This doctrine, although founded in its modern sense by Lamarck, in the early part of the nineteenth century, lay dormant until Darwin, in 1859, brought a new feature into its discussion, by emphasizing the factor of natural selection. The general acceptance of the doctrine, which followed after fierce opposition, had, of course, a profound influence on embryology. The latter science is so intimately concerned with the genealogy of animals and plants, that the newly accepted doctrine, as affording an explanation of this genealogy, was what was most needed. The development of organisms was now seen in the light of ancestral history; rudimentary organs began to have meaning as hereditary survivals, and the whole process of development assumed a different aspect. This doctrine supplied a new impulse to the interpretation of nature at large, and of the embryological record in particular. The meaning of the embryological record was so greatly emphasized in the period of Balfour, that it will be commented upon under the next division of our subject.

The period between Von Baer and Balfour proved to be one of great importance on account of the general advances in knowledge of

all organic nature. Observations were all moving towards a better and more consistent conception of the structure of animals and plants. A new comparative anatomy, more profound, and richer in meaning than Cuvier's, was arising. The edifice on the foundation of Von Baer's work was now emerging into recognizable outlines.

The Period of Balfour, with an Indication of Present Tendencies.

The workers of this period inherited all the accumulations of previous efforts, and the time was ripe for a new step. Observations on the development of different animals—vertebrates and invertebrates—had accumulated in great number, but they were scattered through technical periodicals, transactions of learned societies, monographs, etc., and there was no compact science of embryology with definite outlines. Balfour reviewed all this mass of information, digested it, and molded it into an organized whole. The results were published in the form of two volumes with the title of 'Comparative Embryology.' This book of 'almost priceless value' was given to the world in 1880–81. It was a colossal undertaking, but Balfour was a phenomenal worker. Before his untimely death at the age of thirty-one, he had been able to complete this work and to produce, besides, a large number of technical researches. The period of Balfour is taken arbitrarily in this paper, as beginning about 1874, when he published with Michael Foster 'The Elements of Embryology.'

Balfour was born in 1851. During his days of preparation for the university he was a good student, but did not exhibit in any marked way, the powers for which later he became distinguished. At Cambridge, his distinguished teacher, now Sir Michael Foster, recognized his great talents, and encouraged him to begin work in embryology. After his work in this field was once begun, he threw himself into it with great intensity. He rose rapidly to a professorship in Cambridge, and so great was his enthusiasm and earnestness as a lecturer, that in seven years 'voluntary attendance on his classes advanced from ten to ninety.' He was also a stimulator of research, and at the time of his death there were twenty students engaged in his laboratory, on problems of development.

He was distinguished for personal attractiveness, and those who met him were impressed with his great sincerity, as well as his personal charm. He was welcomed as an addition to the select group of distinguished scientific men of England, and a great career was predicted for him. Huxley, when he felt the call, as a great personal sacrifice, to lay aside the more rigorous pursuits of scientific research, and to devote himself to molding science into the lives of the people, said of Balfour: 'He is the only man who can carry out my work.'

But that was not destined to be. The story of his tragic end need be only referred to. After completing the prodigious labor on

the 'Comparative Embryology' he went to Switzerland for recuperation, and met his death, with that of his guide, by slipping from an Alpine height into a chasm. His death occurred in July, 1882. His portrait is shown in Fig. 11.

The memorial edition of his works fills four quarto volumes, but the 'Comparative Embryology' is Balfour's monument, and will give him enduring fame. It is not only a digest of the work of others, but contains, also, general considerations of a far-seeing quality. He saw developmental processes in the light of the hypothesis of organic evolution. His speculations were sufficiently reserved and nearly always luminous. It is significant of the character of this work to say that the speculations contained in the papers of the rank and file of embryological workers, for more than two decades, and often fondly believed to be novel, were for the most part anticipated by Balfour, and also better expressed, with better qualifications.

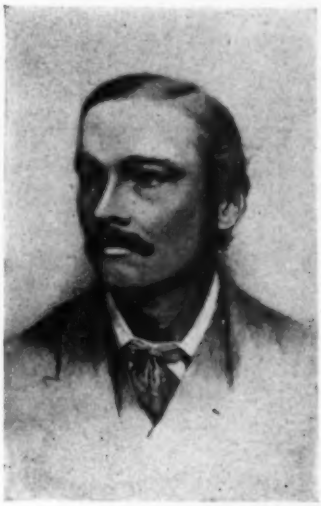

FIG. 11. FRANCIS M. BALFOUR (1851–1882).

The reading of ancestral history in the stages of development is such a characteristic feature of the embryological work of Balfour's period that some observations concerning it will now be in place.

Interpretation of the Embryological Record.—Perhaps the most impressive feature of animal development is the series of similar changes through which all pass in the embryo. The higher animals, especially, exhibit all stages of organization from the unicellular fertilized ovum to the fully formed animal so far removed from it. The intermediate changes constitute a long record, the possibility of interpreting which has been a stimulus to its careful examination.

Meckel, in 1821, and later Von Baer, indicated the close similarity between embryonic stages of widely different animals; Von Baer, indeed, confessed that he was unable to distinguish positively between a reptile, bird and mammalian embryo in certain early stages of growth. In addition to this similarity—which is a constant feature of the embryological record—there is another one that may be equally significant, viz., in the course of embryonic history, sets of rudimentary organs arise and disappear. Rudimentary teeth make their appearance in the embryo of the whalebone whale, but they are transitory and soon disappear without having been of service to the animal. In the embryos of all higher vertebrates, as is well known, gill-clefts and gill-arches, with an appropriate circulation, make their appearance, but

disappear long before birth. These indications, and similar ones, must have some meaning.

Now whatever qualities an animal exhibits after birth are attributed to heredity. May it not be that all the intermediate stages are also inheritances, and, therefore, represent phases in ancestral history? If they be, indeed, clues to ancestral conditions, may we not, by patching together our observations, be able to interpret the record, just as the history of ancient peoples has been made out from fragments in the shape of coins, vases, implements, hieroglyphic inscriptions, etc.?

The results of reflection in this direction led to the foundation of the *recapitulation theory*, according to which animals are supposed, in their individual development, to recapitulate to a considerable degree phases of their ancestral history. This is one of the widest generalizations of embryology. It was suggested in the writings of Von Baer and Louis Agassiz, but received its first clear and complete expression in 1863, in the work of Fritz Müller.

Although the course of events in development is a record, it is, at best, only a fragmentary and imperfect one. Many stages have been dropped out, others are unduly prolonged or abbreviated, or appear out of chronological order, and, besides this, some of the structures have arisen from adaptation of a particular organism to its conditions of development, and are, therefore, not ancestral at all, but, as it were, recent additions to the text. The interpretation becomes a difficult task which requires much balance of judgment and profound analysis.

The recapitulation theory was a dominant note in all Balfour's speculations, and in that of his contemporary and fellow-student, Marshall. It has received its most sweeping application in the works of Ernst Haeckel.

Widely spread through the recent literature is to be noted a reaction against the too wide and unreserved application of this doctrine. This is to be naturally expected, since it is the common tendency in all fields of scholarship, to demand a more critical estimate in research, and to undergo a reaction from the earlier crude and sweeping conclusions.

Improvement in Methods.—Another feature of the work in Balfour's period was increasing attention to methods of preparing material for study. The great problem is to bring tissues under observation, with the normal relations as little disturbed as possible, so that the prepared material will represent the conditions existing in life and no others. "Many of the most important elements of cell-structure are invisible in life, and can only be brought to view by means of suitable fixation, staining and clearing." One great danger is that pseudo-structures will be artificially formed by the action of reagents. On this account great attention has been given to every

feature of technique, and the success of an investigation may depend, very largely, on the care exercised in the use of reagents and dyes, and the mechanical part of getting sections in shape for observation. Investigations of an earlier period were now repeated with greater refinement of technique, and the result was, a change not only in interpretations, but often in the points of observation.

Establishment of Marine Biological Laboratories.—Among other influences which have contributed to the advancement of embryology—as well as to all biology—has been the establishment of fully equipped sea-side laboratories. These have supplied facilities for working where developing forms are most abundant and most diversified. Also, as distributors of prepared material, they have made a wide range of forms available to investigators. The famous '*Stazione Zoologica,*' founded by Dohrn in 1872, and still under his direction, has exercised a powerful influence. Not only have numerous researches in embryology been carried on there, but, also, prepared material has been shipped to investigators in all parts of the world. Balfour was one of the earliest to avail himself of the opportunities at the Naples Station. The Marine Biological Station at Wood's Hole, Mass., of which Whitman has been director since its foundation, in 1886, is to be mentioned as second only to that of Naples for the extent of its influence and quality of its work. The many other similar laboratories in this country and abroad have aided in the great advance along embryological lines.

Nearly all problems in anatomy and structural zoology are approached from the embryological side, and, as a consequence, the work of the great army of anatomists and zoologists has been in a measure embryological. Many of them have produced beautiful and important work, but the work is too extended to admit of review in this connection.

Oscar Hertwig, of Berlin, whose portrait is shown in Fig. 12, is one of the representative embryologists of Europe, and lights of the first magnitude in this country are Brooks, Minot, Whitman and E. B. Wilson.

Although no attempt is made to review the researches of the recent period, we can not pass entirely without mention the discovery of chromosomes and of their reduction in the ripening of the egg and in the formation of sperms. This has thrown a flood of light on the phenomena of fertilization, and has led to the recognition of these bodies as, probably, the bearers of heredity.

The work of the late Wilhelm His, whose portrait is shown in Fig. 13, is also deserving of especial notice. His luminous researches on the development of the nervous system, the origin of nerve fibers, and his analysis of the development of the human embryo are all very important.

Recent Tendencies. Experimental Embryology.—Soon after the publication of Balfour's great work on 'Comparative Embryology,' a new tendency in research began to appear which led onward to the establishment of experimental embryology. All previous work in this field had been concerned with the structure or architecture of organisms, but now the physiological side began to receive attention. Whitman has stated with great aptness the interdependence of these two lines of work as follows: "Morphology raises the question, How came the

FIG. 12. OSKAR HERTWIG IN 1890.

organic mechanism into existence? Has it had a history, reaching its present stage of perfection through a long series of gradations, the first term of which was a relatively simple stage? The embryological history is traced out, and the paleontological records are searched, until the evidence from both sources establishes the fact that the organ or organism under study is but the summation of modifications and elaborations of a relatively simple primordial. This point settled, physiology is called upon to complete the story. Have the functions remained the same through the series? or have they undergone a series of modifications, differentations and improvements more or less parallel with the morphological series?"

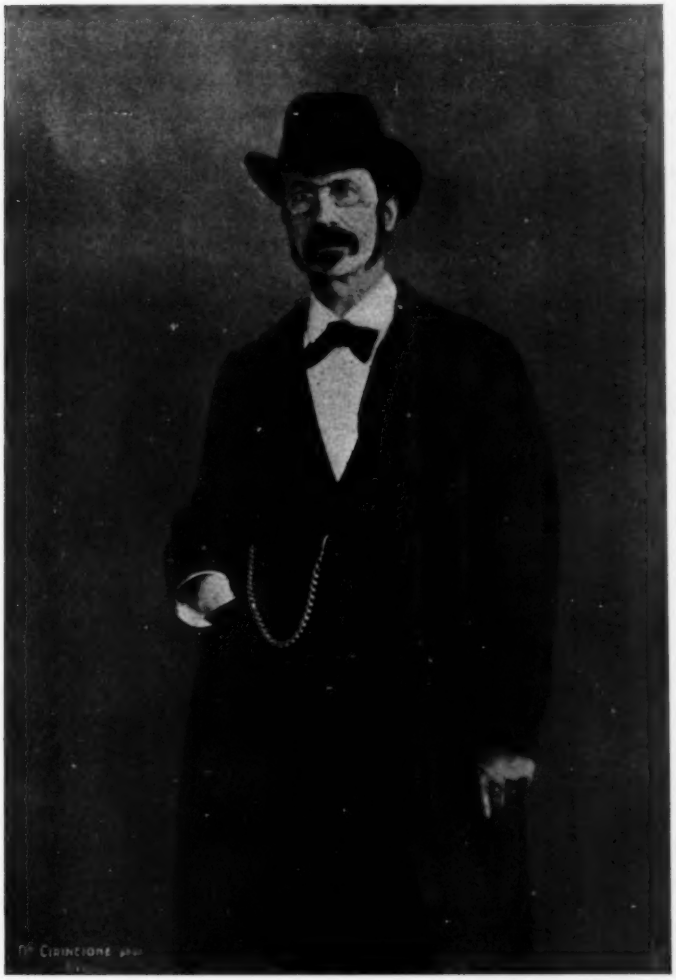

FIG. 13. W. HIS AT SIXTY-FOUR YEARS (1831–1904).

Since physiology is an experimental science, all questions of this nature must be investigated with the help of experiments. Organisms undergoing development have been subjected to changed conditions, and their responses to various forms of stimuli have been noted. In the rise of experimental embryology we have one of the most promising of the recent departures from the older aspects of the subject. The results already attained in this attractive and suggestive field make too long a story to justify its telling in this paper. Roux, Herbst, Loeb, Morgan, E. B. Wilson and many others have contributed to the growth of this new division of embryology. Good reasons have been adduced for believing that qualitative changes take place in the protoplasm as development proceeds. And a curb has been put upon that 'great

fault of embryology, the tendency to explain any and every operation of development as merely the result of inheritance.' It has been demonstrated that surrounding conditions have much to do with individual development, and that the course of events may depend largely upon stimuli coming from without, and not exclusively on an inherited tendency.

Cell-Lineage.—Investigations on the structural side have reached a high grade of perfection in studies on cell-lineage. The theoretical conclusions embodied in the germ-layer theory are based upon the assumption of identity in origin of the different layers. But the lack of agreement among observers, especially in reference to the origin of the mesoderm, made it necessary to study more closely the early developmental stages before the establishment of the germ-layers. It is a great triumph of exact observation that, although continually changing, the consecutive history of the individual cells has been followed, from the beginning of segmentation, to the time when the germ-layers are established. Some of the beautifully illustrated memoirs in this field are highly artistic. Blochman (1882) was a pioneer in observations of this kind, and, following him, a number of American investigators have pursued studies on cell-lineage with great success. The work of Whitman, Wilson, Conklin, Kofoid, Lillie, Mead and Castle has given us the history of the origin of the germ-layers, cell by cell, in a variety of animal forms. These studies have shown that there is a lack of uniformity in the origin of, at least, the middle layer, and therefore there can be no strict homology of its derivatives. This makes it apparent that the earlier generalizations of the germ-layer theory were too sweeping, and this theory is retained in a much modified form.

Theoretical Discussions.—Certain theoretical discussions, based on embryological studies, have been rife in recent years. And it is to be recognized without question, that discussions regarding heredity, regeneration, the nature of the developmental process, the question of inherited organization within the egg, or germinal continuity, etc., have done much to advance the subject of embryology.

Embryology is one of the three great departments of biology which, taken in combination, furnish us with a knowledge of living forms along lines of structure, function and development. The embryological method of study is of increasing importance to comparative anatomy and physiology. Formerly it was entirely structural, but is now becoming, also, experimental, and will be of more service to physiology. While it has a strictly technical side, the science of embryology must always remain of interest to intelligent people as embracing one of the most wonderful processes in nature—the development of a complex organism from the single-celled condition, with a panoramic representation of all intermediate stages.

GALILEO.

BY EDWARD S. HOLDEN, SCD., LL.D.,
U. S. MILITARY ACADEMY.

IV.

WHEN the master of the palace examined the published book he discovered that Galileo had not obeyed the orders and injunctions given to him by the Holy Office on February 26, 1616, sixteen years previously. Therefore the *imprimatur* for Rome was wrongly attached. Galileo did not inform the Inquisitor at Florence of the aforesaid injunctions and orders. Therefore the *imprimatur* for Florence was obtained by 'ruse.' Such was substantially the theory held by Galileo's judges at Rome. It was, in strictness, true. The command of the Holy Office (February 26, 1616) not to hold, teach or defend the Copernican opinion had been violated in the *Dialogues* (as indeed it had been violated less flagrantly in *Il Saggiatore* and in the letter on the tides). The orders of Riccardi were obeyed in form but not in substance. If the text of the *Dialogues* had been submitted at Rome, the Roman *imprimatur* would never have been given.

Finally, the general prohibition of March 5, 1616, not to teach the Copernican opinion had been disobeyed in the *Dialogues*, as in the two preceding publications. That no proceedings had been taken regarding the two last-named books did not in their eyes excuse the issuance of the former.

If Galileo had merely desired to promulgate the Copernican truths it would have been perfectly easy and safe for him to have printed his book in Germany, with or without his name. But he wished for an Italian triumph even more than for the spreading of a doctrine that he knew to be true.

The *Dialogues* were received on all hands with the greatest interest. Galileo's friends were delighted as they before had been with *Il Saggiatore*. They expected a similar reception for his new book, and Galileo beyond a doubt shared their expectations. Castelli—who was in favor with the Pope, and in Rome—wrote that he should read nothing else but the *Dialogues* and his Breviary. The enemies of Galileo were for the moment paralyzed with anxiety and rage. The arguments of the *Dialogues* were more dangerous than those of *Il Saggiatore* even. Its attack on Aristotelianism and orthodoxy was even more insidious and vigorous. The upper classes of Italy have always

keenly relished irony and sarcasm. They were now laughing openly at the overthrow of the scholastics. The universities, the Jesuits and many of the clergy, on the other hand, were solidly arrayed against Galileo. The Jesuits were especially inimical. In a juncture like this everything depended upon the Pope. Galileo confidently expected his support, but he had misread the Pope's mind from the very first. The Pope was surrounded by Galileo's enemies. Every point that would tell was made against the book and its author. The dangers that lurked in the Copernican doctrine were exposed; Galileo's former interpretations of Holy Writ were set forth as monstrous, coming, as they did, from the pen of a layman; their obvious weaknesses were pointed out; he was denounced as a rebel to church authority, which had forbidden any one to teach the Copernican doctrine (March 5, 1616); the Pope was convinced that Galileo had intended to portray him in the character of Simplicius.

It is absolutely certain that Galileo had no such intention. Under the circumstances it would have been madness for him to alienate his powerful friend and patron. Exactly why he closed his *Dialogues* with the quotation of the Pope's own words (spoken to Galileo in 1624) it is impossible to say. To us, in the light of events, the quotation seems an inconceivable blunder. But Galileo was very far from a blunderer. He was skilled in fine logic and with his pen. The closing words of the *Dialogues* (containing the quotation) can be read so as to express a humble submission to authority. It was beyond a doubt, Galileo's intention that they should be so read; it is equally certain that the submission was only perfunctory; the reckless irony of all that preceded them made the quoted words appear as mere foolishness in the mouth of the foolish Simplicius. The very name—Simplicius—was offensive to the Pope. It was not until after July, 1636, that he expressed himself as convinced that Galileo had intended no disrespect. It was then too late. On July 26, 1636, Galileo writes: "I hear from Rome that his Eminence Cardinal Antonio Barberini and the French ambassador (de Noailles) have seen his Holiness and tried to convince him that I never had the least idea of perpetrating so sacrilegious an act as to make game of his Holiness, as my malicious foes have persuaded him, which has been the prime cause of all my troubles." The *prime* cause was Urban's conviction that Galileo had brought scandal into the church by teaching a doctrine which was, as yet, unproved.

The storm was about to break. From now onward the story is fully told in the official documents of the inquisition. The further sale of the *Dialogues* was prohibited. Galileo's conduct was referred to a special commission of theologians and men versed in science to investigate. That it was not directly sent to the Holy Office was a

signal mark of favor. A letter, drawn up by Galileo, was despatched by the Grand Duke to the angry Pope. On September 4, 1632, the Pope said to the Tuscan ambassador, Niccolini—Galileo's faithful friend: 'Your Galileo has ventured to meddle with things that he ought not, and with the most important and dangerous subjects.' He added that Galileo's book had been printed by a ruse. As to the objections to the book 'Galileo knows well enough what the objections are . . . because we have talked to him about them, and he has heard them all from us.' The Pope had acted, he said 'with the greatest consideration for Galileo,' and added that his own conduct towards Galileo had been far better than Galileo's to him, for Galileo had deceived him. The Pope was firmly convinced that religion had been imperiled.

The special commission reported after about a month that Galileo has transgressed orders in deviating from hypothetical treatment of the Copernican opinion and by decidedly maintaining it he has erroneously ascribed the phenomena of the tides to the stability of the sun and the motion of the earth, which do not exist; he has been deceitfully silent about the command laid upon him by the Holy Office in 1616, to relinquish the Copernican doctrine 'nor henceforth to hold, teach or defend it in any way whatsoever, verbally or in writing, etc.,' 'which injunction Galileo acquiesced in and promised to obey.' Furthermore, Galileo printed the *imprimatur* of Rome on the title page of the *Dialogues* without authority; he put the saving clause of the book in the mouth of a simpleton, etc. (A full account of this report is given in Gebler's 'Galileo,' English edition, pp. 172–3. It is only incidentally of importance to us here.)

On the fifteenth of September, 1632, the Pope notified Niccolini that Galileo's affair was to be transferred to the inquisition. This was astounding news to the ambassador, who had all along believed that no proceedings would be taken against the astronomer and that the very worst to be feared was perhaps a command to alter certain phrases of the book. In the interview the Pope said 'Galileo was still his friend'—but that the Copernican opinion had been condemned sixteen years previously. At a meeting of the Congregation of the Holy Office held on September 23, it was pronounced that Galileo had disobeyed the command of February 26, 1616, and had concealed the prohibition then received by him from the censor at the time he applied for the *imprimatur* for his book; the inquisitor at Florence was, on the same day, by command of the Pope, directed to summon Galileo to appear before the commissary-general of the Holy Office in Room, 'as soon as possible, in the course of the month of October.' On October 1, Galileo, in writing, acknowledged the receipt of the summons and promised to present himself during October, as directed.

The correspondence of Galileo shows that the summons came as a complete surprise to him, and he could not have received it without grave apprehension. He had risked everything in the belief that the Pope's favor and friendship would continue; but it is plain that this order would never have been despatched unless that favor had been withdrawn; his enemies had triumphed; he was at the mercy of men who would show no mercy to him personally—as in times past he had shown no mercy to them; even his friends among the Roman notabilities were powerless in the face of the Pope's anger; and his most influential supporter—Prince Cesi—was dead. There can have been no moments in all of Galileo's long life so bitter as these. The whole fabric he had built up in his imagination crumbled in an instant. Numberless incidents that he had formerly interpreted in one way must have arisen in his mind demanding new and more veracious interpretations that could be reconciled with the present bewildering reversal of all his hopes and beliefs. The Holy Office would have no difficulty in proving him culpable of disobedience to its orders; the general prohibition binding on all catholics he had openly disobeyed, as well as the prohibitions special to his case.

A letter written on October 13 to one of the cardinals, Barberini,* shows Galileo's consternation and astonishment. He curses the time, he says, devoted to his studies. He begs the cardinal to intercede with the wise fathers in Rome, not to release him from giving an account of himself, which he is ready and anxious to do—but to make it easiest for him to obey. He can give his account in two ways; he can write a full history of his whole connection with the Copernican controversy which will prove to any one free from party malice that he has all along acted piously and as a good catholic; or he can give it verbally to the officers of the Inquisition in Florence. If, however, no dispensation or delay can be granted he will make the journey to Rome in spite of his great age and many bodily infirmities. The Tuscan ambassador at Rome interested himself in the matter, and throughout the whole of Galileo's process was devoted, prudent, wise and unwearied. No son could have been more faithful, nor more delicate. The letter was delivered, but the Pope would not permit delay. Galileo must come to Rome to answer. Niccolini then appealed directly to the Pope, begging for delay on account of Galileo's infirmities. The answer was that he must come—slowly, if necessary—with every comfort—but he must be tried in person, 'for having been so deluded as to involve himself in these difficulties, from which we had relieved him when we were cardinal.' On the ninth of December orders were sent to Florence to compel Galileo to set out. A medical

* Cardinal Antonio Barberini senior was the brother, and Cardinal Antonio junior was the nephew, of the Pope.

certificate of the seventeenth by Galileo's physicians pronounced him unfit to travel. The certificate was not believed in Rome, and Niccolini reported on the thirtieth that it was intended to send a physician from Rome with a commissioner who would, if he were fit to travel, bring him to Rome in chains.

On January 11, 1633, the Grand Duke wrote to Galileo advising him to set out, offering him one of the Court litters to travel in, and the hospitality of the ambassador's palace in Rome. On the twentieth of January Galileo left Florence on his last journey to Rome, arriving there, after a tedious quarantine, on February 13. Galileo, though technically a prisoner, was permitted to reside at the ambassador's palace. He writes to the Tuscan secretary of state that his treatment indicates 'mild and kindly treatment very different from the threatening words, chains and dungeons.' He was allowed to drive out, the shades of the carriage being half-drawn. His letters show that he was full of hope. It was now more than four months since he had been cited to appear, and in this time he must have considered what form the charges were to take and what defense he should make. Niccolini's despatch of February 27, 1633, says:

> The main difficulty consists in this—that these gentlemen maintain that in 1616 he [Galileo] was ordered neither to discuss the question [the Copernican opinion] nor to converse about it. He says, on the contrary, that these were not the terms of the injunction which were that that doctrine was not to be held or defended. He considers that he has the means of justifying himself, because it does not at all appear from his book that he does hold or defend the doctrine nor that he regards it as a settled question, as he merely adduces the reasons, *hinc hinde*. The other points appear to be of less importance and easier to get over.

From this despatch of Galileo's friend it appears that his defense was settled upon. The certificate of Cardinal Bellarmine was to be submitted to his judges; and it was to be proved from his book that he had obeyed the orders of the cardinal. Nothing was left undone by Niccolini, Castelli, or by the Grand Duke, to forward Galileo's interests. The Duke wrote letters of recommendation to the ten cardinals who made up the Holy Office, and some of the cardinals read the *Dialogues* and discussed them with Castelli. On April 12 Galileo was cited to appear at the Palace of the Inquisition. He acknowledged the *Dialogues* to be his own work. He was then asked to recount the proceedings of 1616 and replied that Cardinal Bellarmine had then told him 'that the aforesaid opinion of Copernicus might be held as a conjecture, as it had been held by Copernicus, and his eminence was aware that, like Copernicus, I only held that opinion as a conjecture,' which is evident from a letter (dated April 12, 1615) from the cardinal to Foscarini, in which he says: "It ap-

pears to me that your Reverence and Signor Galileo act wisely in contenting yourselves with speaking *ex suppositione* and not with certainty."

"In the month of February, 1616, Signor Cardinal Bellarmine told me that as the opinion of Copernicus, if adopted absolutely, was contrary to Holy Scripture, it must neither be held or defended, but that it might be held hypothetically and written about in this sense." Here Galileo presented a copy of the certificate which declares that the doctrine of Copernicus 'is contrary to the Holy Scriptures and therefore can not be defended or held.' The Inquisition then asked if any other command was communicated to him and if he would remember it, if what was then said was read aloud to him. Galileo replied: "I do not remember that anything else was said or enjoined upon me, nor do I know that I should remember what was said to me, even if it were read to me. I say freely what I do remember, because I do not think that I have in any way disobeyed the injunction, that is, have not by any means held or defended the said opinion that the earth moves and the sun is stationary." The Inquisition now remind Galileo that a command was issued to him, before witnesses, enjoining "that he must neither hold, defend nor teach that opinion in any way whatsoever.' The annotation commands Galileo to 'relinquish altogether' the Copernican opinion, and forbids him 'henceforth to hold, teach or defend it in any way whatsoever, verbally or in writing; otherwise proceedings would be taken against him in the Holy Office; which injunction the said Galileo acquiesced in and promised to obey."

The Inquisition asks if Galileo remembers how and by whom the words first quoted were intimated to him. He replies: "I do not remember that the command was intimated to me by anybody but by the cardinal verbally; and I remember that the command was 'not to hold or defend.' It may be that 'and not to teach' was also there. I do not remember it, neither the definition 'in any way whatsoever,' but it may be that it was, for I thought no more about it, nor took any pains to impress the words on my memory, as a few months later I received the certificate now produced of the said Signor Cardinal Bellarmine, of twenty-sixth May, in which the injunction 'not to hold or defend' that opinion is expressly to be found. The two other definitions of the said injunction that have just been made known to me, namely, 'not to teach,' and 'in any way,' I have not retained in my memory, I suppose, because they are not mentioned in the said certificate on which I rely and which I have kept as a reminder."

Emphasis is laid by Gebler in his *Galileo* on the difference between an injunction 'not to teach' and one 'not to hold or defend.' I can see no essential difference between forbidding a citizen of Russia, let

us say, from holding or defending anarchistic opinions and forbidding him from holding, teaching or defending such opinions in any way whatsoever, verbally or in writing. The latter prohibition is more formal. It is not more absolute. The annotation of February 26, 1616, is received throughout the process by the Inquisitors as exact in all particulars. *It is not denied by Galileo;* he says merely that he does not recall certain parts of it. It does not formally appear that the witnesses to it were called to testify. If they had been called their recorded testimony would have settled certain points that must now be settled from the text of the annotation itself. I can see no reason to doubt that the words of the text mean precisely what they say.

This is perhaps the place to say that the documents of Galileo's process have been examined again and again and that each examination has proved that the papers have not been tampered with in any manner and that they represent the case as it was understood by the Holy Office with minute accuracy. The hearing for the first day was closed with further questions and answers. Galileo was asked whether after the aforesaid command was issued to him he received permission to write the *Dialogues.* He replied: "After receiving the command aforesaid I did not ask permission to write the book . . . because I did not consider that in writing it I was acting contrary to, far less disobeying, the command not to hold, defend, or teach, the said opinion." The next questions relate to the printing of the book and Galileo is asked if he had informed the censor of the command aforesaid. He replies: "I did not say anything about the command to the master of the palace . . . for I have neither maintained nor defended the opinion that the earth moves and the sun is stationary in that book, but have rather demonstrated the opposite of the Copernican opinion and shown that the arguments of Copernicus are weak and not conclusive." Galileo's defense is here outlined. It is to be that he did not 'hold' the Copernican opinion after 1616. Not holding it, he did not defend it, nor teach it. Hence he had disobeyed no command, he maintains, although it is obvious to all that the *Dialogues,* like his other writings, are a brilliant defense of the system of Copernicus.

An apartment of 'three large and comfortable rooms' was assigned to Galileo in the Palace of the Holy Office, as he was their prisoner. His servants stayed with him. His meals were sent in by the devoted Niccolini, to whom he wrote every day with perfect freedom. His own account of the proceedings of the first day of his examination is as follows:

> I arrived in Rome on the tenth of February and I was placed in the clement charge of the Inquisition and of the Sovereign pontiff, Urban VIII., who esteemed me although I could not rhyme epigrams and little love-sonnets. I

was placed in arrest in the delicious palace of . . . the Ambassador of Tuscany. The next day I received the visit of P. Lancio, Commissary of the Holy Office, who took me with him in his carriage. On the way he questioned me, showing a great desire that I should repair the scandal I had raised throughout all Italy by maintaining the opinion of the motion of the earth. To all the mathematical reasons that I could bring forward he answered one thing only. *Terra autem in æternum stabit, quia terra in æternum stat*, as the Scripture saith. Thus discoursing, we arrived at the palace of the Holy Office. I was presented, by the commissary, to the assessor with whom I found two Dominican monks. They notified me, with civility, that I should be permitted to explain my reasons to the congregation, and that, subsequently, my excuses would be heard if I were judged culpable. The following Sunday I appeared, in fact, before the congregation and proceeded to set forth my proofs. To my ill-fortune they were not satisfying; no matter what pains I took I could not succeed in making myself understood. My arguments were interrupted by their zeal, they spoke only of the scandal I had caused, always bringing up the passage of Scripture referring to the miracle of Joshua, as the unanswerable portion of the matter. This reminded me of another passage in which the language of the Bible is entirely conformable to popular notions—(The heavens are solid and polished like a mirror of brass). This example seemed to me to be opposite to prove that the words of Joshua could be so interpreted and the conclusion seemed to me to be entirely just. But they gave it no weight and I was answered only by shrugging of shoulders.

Galileo's own account of the proceedings gives a different impression from that of the official record. He was argumentative about texts of Scripture, and when his explanation of Joshua's miracle was not found satisfactory, he suddenly recalls another text which will convince the Inquisitors, he thinks, that Scripture is not to be interpreted literally. They answered by shrugging their shoulders and by again referring to the scandal he has created in the Church. Galileo does not seem to have, even yet, realized the situation. A letter from the commissary-general of the Inquisition to Cardinal Francesco Barberini (dated April 28, 1633) explains the events of the next weeks. The letter states that the commissary has informed the cardinals of the Holy Office regarding Galileo's case, and that they "took into consideration various difficulties with regard to the manner of pursuing the case and of bringing it to an end. More especially as Galileo has in his examination denied what is plainly evident from the book written by him; since in consequence of this denial there would result the necessity for greater rigor of procedure and less regard to other considerations belonging to this business. Finally I suggested a course, namely, that the Holy Congregation should grant me permission to treat extra-judicially with Galileo, in order to render him sensible of his error, and to bring him, if he recognizes it, to a confession of the same . . . permission was granted me. That no time might be lost, I entered into discourse with Galileo yesterday afternoon, and after many arguments and rejoinders had passed between

us, by God's grace I attained my object, for I brought him into a full sense of his error. . . . The affair is being brought to such a point that it may soon be settled without difficulty. The court will maintain its reputation; it will be possible to deal leniently with the culprit. . . . "

Who can say what the arguments of the commissary of the inquisitor were? They were effective. Galileo's attitude was utterly and instantly changed. On the thirtieth of April he again appeared before the Holy Office and read the following confession:

In the course of some days continuous and attentive reflection . . . it occurred to me to reperuse my printed dialogue, which for three years I had not seen, in order carefully to note whether, contrary to my most sincere intention, there had, by inadvertence, fallen from my pen anything from which a reader or the authorities might infer not only some taint of disobedience on my part but also . . . that I had contravened the orders of the Holy Church. . . . I freely confess that in several places it seemed to me set forth in such a form that a reader ignorant of my real purpose might have had reason to suppose that the arguments adduced on the false side, which it was my intention to confute, were so expressed as to be calculated rather to compel conviction by their cogency than to be easy of solution.

Two arguments there are in particular—one taken from the solar spots, the other from the ebb and flow of the tide—which in truth, come to the ear of the reader with far greater show of force and power than ought to have been imparted to them by one who regarded them as inconclusive, and who intended to refute them, as I truly and sincerely held and do hold them to be inconclusive and admitting of refutation.

And, as excuse to myself for having fallen into an error so foreign to my intention, not contenting myself entirely with saying that when a man recites the arguments of the opposite side with the object of refuting them, he should, especially if writing in the form of dialogue, state these in their strictest form, and should not cloak them to the disadvantage of his opponents—not contenting myself, I say, with this excuse—I resorted to that of the natural complacency which every man feels with regard to his own subtleties and in showing himself more skilful than the generality of men, in devising them, even in favor of false propositions, ingenious and plausible arguments. With all this, although with Cicero's '*avidior gloriæ quam satis est*' if I had now to set forth the same reasonings, without doubt I should so weaken them that they should not be able to make an apparent show of that force of which they are really and essentially devoid. My error, then, has been—and I confess it—one of vainglorious ambition, and of pure ignorance and inadvertence.

This is what it occurs to me to say with reference to this particular, and which suggested itself to me during the reperusal of my book.

This confused and almost incoherent confession is totally unlike the precise and elegant phrases of Galileo's writings. It is a complete reversal of his former position. Parts of it are evidently mere reminiscences of his conversation with the commissary-general ('vainglorious ambition,' for instance, is a phrase that he must have accepted, not one originating with himself). The whole is a weak abandonment of a position proudly held and is as different as possible from the

manly attitude of Cremonini—an attitude, be it remarked, which he successfully maintained in the face of the Inquisitors. No one can read it without pity. It can be interpreted in many differing ways. My own interpretation is that Galileo was persuaded to make the confession by representations that the case was very serious indeed and that a general admission of the sort would satisfy the Pope and cardinals; and that after the confession was obtained it was not very difficult for his judges to proceed to the abjuration; while if the abjuration had been first proposed Galileo might have desperately refused to make it, thus precipitating a crisis most unwelcome to the Holy Office. This is mere conjecture and is perhaps not worth recording. Certain it is that, the confession once extorted, all the dignity of Galileo's attitude was lost. By a slight increase of pressure one who had already yielded so much could be made to yield more, and finally to yield all. It seems to be clear that the pressure was gradually applied.

The confession was received by the congregation. Galileo withdrew; but almost immediately returned to offer to write a continuation of his *Dialogues* which should most effectually confute the arguments of the earlier portions. This offer is interpreted by Gebler as 'weakness and insincere obsequiousness.' It appears to me to be simply an attempt on his part to prevent the condemnation and prohibition of his book; and to show that he was, even yet, far from realizing the grimness of the situation. Immediately ofter the hearing, Galileo, still a prisoner of the Inquisition, was permitted to return to the palace of the Tuscan ambassador. He wrote letters (which are not extant) to friends. Their answers show that he 'entertained the most confident hopes of a successful and speedy termination of his trial.' One of them writes (May 12) from Florence: "I have for a long time had no such consolatory news as that which your letter of the seventh brought me. It gives me well-founded hopes that the calumnies and snares of your enemies will be in vain . . . since you have gained far more than you have lost by the calamity that has fallen upon you. My pleasure is still more enhanced by the news that you expect to be able to report the end of the affair in your next letter."

On May 10, Galileo was again summoned and was informed that eight days would be allowed him to prepare a defense. He, however, had already prepared it and at once submitted the following:

When asked if I had signified to the R. P. the Master of the Palace the injunction privately laid upon me, about sixteen years ago, by orders of the Holy Office, not to hold, defend or 'in any way' teach the doctrine of the motion of the earth and the stability of the sun, I answered that I had not done so. And not being questioned as to the reason why I had not intimated it, I had no opportunity to add anything further. It now appears to me to be necessary to state the reason in order to demonstrate the purity of my in-

tention, ever foreign to the employment of simulation or deceit in any operation I may engage in. I say, then, that as at that time reports were spread abroad by evil-disposed persons, to the effect that I had been summoned by the Lord Cardinal Bellarmine to abjure certain of my opinions and doctrines, and that I had consented to abjure them, and also to submit to punishment for them, I was thus constrained to apply to his Eminence, and to solicit him to furnish me with a certificate, explaining the cause for which I had been summoned before him; which certificate I obtained, in his own handwriting and it is the same that I now produce with the present document.

From this it clearly appears that it was merely announced to me that the doctrine attributed to Copernicus of the motion of the earth and the stability of the sun must not be held or defended and (here the original MS. is defaced) . . . beyond this general announcement affecting every one, any other injunction in particular was intimated to me, no trace thereof appears there. Having, then, as a reminder, this authentic certificate in the handwriting of the very person who intimated the command to me, I made no further application of thought or memory with regard to the words employed in announcing to me the said order not to hold or defend the doctrine in question; so that the two articles of the order—in addition to the injunction not to 'hold' or 'defend' it—to wit the words 'nor to teach it' 'in any way whatsoever—which I heard are contained in the order intimated to me, and registered—struck me as quite novel and as if I had not heard them before; and I do not think I ought to be disbelieved when I urge that in the course of fourteen or sixteen years I had lost all recollection of them . . . whence it appears to me that I have a reasonable excuse for not having notified to the Master of the Sacred Palace the command privately imposed upon me . . . " [then follows a paragraph declaring that the faults scattered through this book 'have not been artfully introduced' but are inadvertent, owing to a vainglorious ambition and complacency . . . which fault he is ready to correct.]

Lastly, it remains for me to pray you to take into consideration my pitiable state of bodily indisposition to which, at the age of seventy years, I have been reduced by ten months of constant mental anxiety . . . ; [and he hopes that his judges may remit (his punishment) and may defend his honor and reputation against the calumnies of ill-wishers].

No one can read this confession and defence without a feeling of deep pity. This is even intensified if we find in it a lack of entire candor as it is hard not to do—'mistrust in the truthfulness of the accused'—is Gebler's phrase. Galileo returned to his palace feeling that his confession had served him well and that his trial was to come to a favorable issue. His confession had, however, put him in the power of his judges. They believed that now was the time to make a signal example. It was decided by the congregation (June, 1633) to bring Galileo to trial 'as to his intention and under threat of torture.'

On the morning of June 21 Galileo appeared before the Holy Office, and after being sworn was questioned. His first answer was:

A long time ago, that is before the decision of the Holy Congregation of the Index . . . I was indifferent and regarded both opinions, namely that of

Ptolemy and that of Copernicus, as open to discussion, inasmuch as either one might be true to nature; but after the said decision, assured of the wisdom of the authorities, I ceased to have any doubt; and I held and still hold, as most true and indisputable, the opinion of Ptolemy, that is to say, the stability of the earth and the motion of the sun.

Questioned upon the publication of his *Dialogues,* he answers in accordance with his previous utterances. 'I am here to obey,' he says, 'and I have not held this opinion since the decision was pronounced.' The protocol of his trial concludes with the words: (Galileo's) 'signature was obtained to his deposition and he was sent back to his place.' This place was not the palace of the Tuscan ambassador. Galileo was detained at the building of the Holy Office till June 24. It is the opinion of the best judges that Galileo was not confined in the dungeons of the Inquisition.

There is not in the Vatican manuscript of the protocol, or in any other place, any evidence or any hint that Galileo was put to the torture at this or at any time. That he was threatened with the torture is equally certain. If he had boldly professed the Copernican opinion the proceedings would have taken a course that had been prescribed in advance (June 16). As he was disposed to abjure this opinion the course was different.

On the twenty-second of June, 1633, Galileo was brought into the presence of his judges, where his sentence was pronounced. The sentence of Galileo is a long document. The following extracts contain the points of especial importance.

We the undersigned (the names are given), by the Mercy of God, Cardinals of the Holy Roman Church, Inquisitors-general throughout all the Christian Republic, deputed by the Holy Seat against heretical perversity:

Whereas, you, Galileo, son of the late Vincenzio Galilei, Florentine, aged 70 years, *were denounced, in 1615, to this Holy Office, for holding as true a false doctrine* proposed by several authors, *that is to say, that the sun is immovable* . . . ; and moreover for having had certain disciples to whom you taught the same doctrine; for having corresponded on this subject with certain mathematicians of Germany; for having made public certain letters on the subject of spots upon the Sun in which you expounded the said doctrine as true; and whereas you answered, when objections were made to you citing to you passages of Scripture, by explaining the said Scripture in your own manner; and whereas a copy of a letter was shown to you, said to have been written by you to one of your former disciples (Castelli), in which you, still maintaining the hypotheses of Copernicus, interpreted several propositions contrary to the meaning and the authority of Holy Writ:

This Holy Tribunal being therefore desirous of proceeding against the disorder and mischief thence resulting . . . the two propositions of the stability of the Sun and the motion of the earth were . . . qualified as follows:

The proposition that the sun is the center of the world and does not move from its place is absurd and false philosophically, and formally heretical, because it is expressly contrary to the Holy Scripture.

The proposition that the earth is not the center of the world and immovable,

but that it moves, and also with a diurnal motion, is equally absurd and false philosophically, and theologically considered, at least erroneous in faith.

But whereas *at the same time* it was our pleasure to proceed against you with benignity, *it was decided in the Holy Congregation . . . February 25, 1616, that the Very Eminent Cardinal Bellarmine should enjoin you to quit entirely the said false doctrine, not to teach it to others, not to defend it, never to treat it, under penalty that, if you failed to agree to this precept you would be thrown into a prison,* and for the execution of this decree, on the following day, in the Palace, in presence of the said Cardinal Bellarmine, after having been benignly admonished by him, *you received* from the Commissary of the Holy Office, *in the presence of* a notary and of *witnesses the injunction to desist entirely from the said opinion and for the future it was forbidden to you to defend it, or to teach it in any way, whether by word of mouth or by writing; and having promised obedience, you were dismissed . . .* and, *whereas, there appeared last year, at Florence, a book whose title named you as the author . . . in which was found a manifest transgression of the aforesaid ordinance intimated to you, and as in that book you defended the opinion that had been condemned,* although, in the book, by various devices, you endeavored to persuade that you left that opinion undecided and expressly probable, which is in itself a very grave error, since an opinion cannot be probable when it has been declared and defined to be contrary to Holy Writ:

It is for this reason that, by our order, you have been *called to this Holy Office,* where, examined upon oath, *you admitted that the said book was written and published by you; you confessed that it was commenced about twelve years ago,* after having received the injunction above-named, *and that you asked permission to publish it without signifying* to those who were empowered to grant permission, *that you had been enjoined from holding, defending or teaching such doctrine in any manner whatsoever.*

You also confessed that the said book in several places is so written that the arguments in favor of a false opinion may appear to be of a nature to force agreement, rather than such as to be easily refutable; *you excused yourself for falling into an error foreign to your intention* on account of the dialogue form and because of one's natural inclination to show oneself more acute and more subtle than the generality of men. . . .

And whereas delay had been granted you to prepare your defense you produced a letter from Cardinal Bellarmine, that you had obtained from him in order to defend yourself from the calumnies of your enemies who had spread abroad that you had abjured and that you had been punished by the Holy Office. This letter declares that you did not abjure nor were you punished; that you had only been notified of the declaration . . . that the doctrine of the motion of the earth . . . is contrary to the Holy Scriptures and that it can not be held or defended; and that as no mention was made in it of the prohibition of teaching in any manner whatever, it is to be believed that in the course of fourteen or sixteen years, this especial thing escaped your memory, and that this is the reason you said nothing of it when asking permission to print, and that in so speaking, *you do not wish to excuse your error* which should be imputed to a vainglorious ambition rather than to ill intention. But even this certificate, produced in your defense, only makes your cause worse, since it is there said that the said opinion is contrary to Holy Writ, and nevertheless you have dared to treat and defend it, etc., and *the permission* (to print) *that you obtained by ruse cannot help you. . . .*

And as it appeared to us that you did not speak the whole truth concerning your intentions, we judged it necessary to proceed to a rigorous ex-

amination at which . . . you answered like a good Catholic. . . . Therefore, having considered the merits of your case, with your confessions and excuses, and all that ought justly to be seen and considered, we have arrived at the underwritten final sentence against you . . . we say that *you*, the said *Galileo . . . have rendered yourself* . . . vehemently *suspect of heresy* . . . and that consequently you have incurred all the censures and penalties imposed . . . against such delinquents. From which *we are content that you be absolved, provided that first . . . you abjure*, curse and detest the aforesaid errors (and) heresies . . . in the form to be prescribed by us, . . . and we ordain that the book of the *Dialogues* . . . be prohibited by public edict.

We condemn you to the formal prison of this Holy Office during our pleasure, and by way of salutary penance, we enjoin that for three years you repeat the seven penitential psalms once weekly, reserving to ourselves full liberty to moderate . . . the aforesaid penalties . . . [signatures of seven cardinals—three not being present or not signing.]

The abjuration of Galileo is the last document of the pitiable history:

I, Galileo Galilei, . . . aged seventy years, arraigned personally before this tribunal and kneeling before you . . . swear that I have always believed, do now believe and by God's help will for the future believe, all that is . . . taught by the Holy Catholic and Apostolic Roman Church. But whereas—*after an injunction* had been judicially intimated to me . . . *that I must altogether abandon the false opinion* that the sun is the center of the world and immovable, and that the earth is not the center of the world, and moves, and *that I must not hold, defend or teach in any way* whatever, verbally or in writing, *the said doctrine* and *after* it had been notified to me that *the said doctrine was contrary to Holy Writ—I wrote* and printed *a book in which I . . . adduce arguments* of great cogency *in its favor* . . . and for this cause I have been pronounced by the Holy Office to be vehemently suspected of heresy . . . *therefore desiring to remove . . . this strong suspicion*, reasonably conceived against me, with sincere heart and unfeigned faith *I abjure*, curse, and detest the aforesaid errors and heresies . . . and I swear that in future I will never again say or assert verbally or in writing, anything that might furnish occasion for a similar suspicion regarding myself; but that should I known any heretic . . . I will denounce him. . . . I the said Galileo have abjured, sworn, promised and bound myself as above . . . this twenty-second day of June, 1633.

Of the foregoing documents it is necessary to say that most have been translated from the French of Delambre, as the English translations of Gebler were not accessible at the time of writing. It is believed that the extracts given accurately represent the originals. Certain phrases have been printed in italics to emphasize the essential facts of the story.

It is also necessary to inquire whether the documents, as printed, correctly state the facts of the trial of Galileo, his explanations, confessions and abjurations. It was certainly within the power of the writers of them to state these facts falsely, or to place them in a false light. Every one has to make up his mind for himself whether

the foregoing documents are to be taken as correct statements of the circumstances before and during the trial, or not. It is assumed in this paper that they are, in this respect, correct.

It seems impossible to make any thing more than a verbal distinction between an injunction 'not to teach' and one 'not to hold or defend.' An opinion that is held and defended to others is an opinion taught to them.

The words of Galileo's judges appear to mean precisely what they say. There was no need to distort them, for his confession of April 30 placed him completely at the mercy of his judges.

A discussion by Gebler (pp. 234–239) of the legality of the proceedings against Galileo and of the effect of the sentence against him brings out with complete demonstration the propositions that: 'the sentence of Galileo rests again and again, even on the principles of the ecclesiastical court itself, on an illegal foundation'; that 'Roman Catholic posterity can say to this day'—with truth—"that Paul V. and Urban VIII. were in error 'as men' about the Copernican system, but not 'as Popes'"; and that "the conditions which would have made the decree of the congregation, or the sentence against Galileo, of dogmatic importance were wholly wanting. Both Popes had been too cautious to endanger (the) highest privilege of the papacy, by involving their infallible authority in the decision of a scientific controversy."

There can be no doubt of the validity of these conclusions. The purpose of the prosecution was to check the spread of Copernican doctrines among the faithful and to utterly ruin the authority of Galileo. This purpose was fully attained when notice of his abjuration and punishment was sent to all vicars "so that it may come to the knowledge of all professors of philosophy and mathematics . . . that they may understand the gravity of the fault he has committed as well as the punishment they will have to undergo should they (likewise) fall into it." (July 2, 1633).

There is no need to trace the further history of Galileo's life in detail. He was permitted to return to the neighborhood of Florence and there he lived until his death in 1642—the year of Newton's birth.

His friend and pupil Castelli writes of his death:

> The noblest eye which nature ever made is darkened; an eye so privileged, and gifted with such rare powers that it may truly be said to have seen more than the eyes of all that are gone, and to have opened the eyes of all that are to come.

The year 1638 was marked by the publication of his epoch-making book 'Discourses on two new Sciences appertaining to Mechanics and Motion.' This contained the foundation of the modern doctrine of

mechanics and it is the crowning glory of Galileo's life. It attracted instant and universal attention, and at the age of seventy-four Galileo was again recognized by all Europe as a master of science—a founder of doctrine. The troubles of his later years grew light in the satisfaction of his legitimate pride.

Myths have grown up about the history of Galileo that it is not necessary to destroy. The whole distressing story has been told in authentic documents. He never suffered bodily torture; he was humiliated and discredited. He never even dared to whisper: *E pur se muove.* His history, though misinterpreted, has been of the deepest service to the world. It affords a symbol around which the rights of men to freedom of thought have clustered. Just as Benedict Arnold serves as the type of a traitor, so Galileo has been made to serve as a martyr of science. But he was no martyr. A true martyr does not abjure his opinions even in presence of the rack. While his recantation may be excused, it does not testify to moral greatness. We may add a paragraph from Gebler:

> Party interests and passions have to a great extent and with few exceptions, guided the pens of those who have written on Galileo's life. The one side has lauded him as an admirable martyr of science, and ascribed more cruelty to the Inquisition than it really inflicted on him; the other has thought proper to enter the lists as defender of the Inquisition, and to wash it white at Galileo's expense. Historic truth contradicts both.

Galileo was a genius of the first order. His title to lasting fame rests principally on his investigations in mechanics and physics, on the theory of the pendulum, the law of falling bodies, the invention of the thermometer, and on the intelligence with which he employed his unique opportunity for telescopic discoveries. His popular reputation will, however, always be based upon his re-invention of the telescope, his advocacy and proof of the Copernican system, his sufferings from the Inquisition, his torture, his abjuration, his seclusion at Arcetri. He will remain preeminently the martyr for science.

THE TEACHING OF LOGIC.

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IT is a well-known fact that logic is not so generally studied to-day as formerly, and that, on the whole, the attitude towards its educational value is one of indifference. If, however, we try to account for its present status in our colleges by granting that logic is an inherently difficult and uninteresting subject, our explanation is both inadequate and unfair to the subject itself. True, logic does require a distinctively analytic and reflective quality of mind. It does not afford the possibility that some subjects do, of getting through it by leaning upon memory, or by appropriating the thoughts of another; but, on the other hand, it constantly demands a conscious effort to think, in the absence of those substitutes for thinking things out for themselves which the weak and lazy-minded resort to. True, logic can make no exclusive claim to being an intellectual discipline. Other academic studies furnish just as severe tests of mental power. The real truth of the matter is that the formal conception and abstract presentation of logic are responsible for a large share of its unattractiveness and needless difficulties.

What I have to write has reference only to elementary or introductory logic. As to the metaphysics or the higher problems of logic I have nothing to say, in this connection, save to express my firm belief that the less an elementary course in logic has to do with metaphysical questions the better. In fact, the discredit into which logic has fallen is in part due to the teaching of the subject from the philosophic standpoint. To hold the place that it deserves in the college curriculum, logic must be shown to have some practical value. I know that this word is in disfavor. We are told of the mathematician who thanked heaven that he had at last discovered a truth which no one would ever be able to make any use of. Perhaps this seeker after truth was but voicing the common antipathy for the word practical. If it is a hopelessly obnoxious term, why not adopt a word used by President Eliot and then always aim to make truth serviceable? It is my purpose in this article to point out some changes in the mode of presenting logic, whereby it may be modernized and made an attractive and useful undergraduate study.

In the first place, the definition of logic should convey to the be-

ginner's mind a comprehension of its scope and purpose as concrete as possible. I can think of no better way of accomplishing this than by stating at the outset that the business of logic is to formulate and systematically present the methods of our thinking for the purpose of acquiring knowledge of the correct methods and skill in their use. This would make logic a science, treating its subject matter descriptively rather than philosophically.

Then proceeding to what is properly the first division of logic, namely, the study of words or terms, it should be made clear that logic treats language from the standpoint of meaning. In reality, logic, so far as its discussion of terms and propositions extends, is one of the trinity of subjects which have to do with the use of language. Its relation to grammar and rhetoric may be best made clear by regarding as the primary interest of logic the function of words as expressing the thought of the speaker or writer. Of course, logical analysis is inseparable from the correct teaching of grammar and rhetoric. But the actual practise is frequently such as to warrant the criticism that sense is sacrificed to grammatical and rhetorical niceties. One is also reminded of that portion of Mr. Huxley's criticism of the teaching of English literature at Oxford, where he writes: "I venture to doubt the wisdom of attempting to mold one's style by any other process than that of striving after clear and forcible expression of definite conceptions; in which process the Glassian precept, 'first catch your conceptions,' is probably the most difficult to obey."*

If students take up logic with the idea, carried over from their study of grammar and their use of dictionaries, that words get their character as nouns, or verbs, or what not, from their origin or form, they should be made to understand early that it is quite an erroneous idea. "The logical character of a name is not something fixed and stable, but quite the reverse. It is function, not structure, that determines logical character, and the function of words in asserting is variable. The different actual uses of names are what logic needs to distinguish, not different sorts of names apart from their actual use, words in their context, not words as grammar conceives them or as they lie side by side in a dictionary. . . . Since words are adaptable instruments of assertion, and not restricted to a single function, we might as well ask whether a penny stamp in the pocket is a receipt stamp or a postage stamp, as ask whether a word apart from its particular use has this or that logical character."† The logical treatment of terms is essentially the question of how they are used in this or that connection.

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* 'Life and Letters,' vol. 2, p. 302.

† Sidgwick, 'Use of Words in Reasoning,' p. 243.

The same point of view should be maintained when we come to the next division of logic—the study of propositions. Here, likewise, the meaning side of language, and not the form, is of primary interest. Many students have difficulty in realizing that the meaning of a proposition does not depend upon its form; that affirmative, or negative, or categorical forms are not necessarily expressions of affirmative, or negative, or categorical thoughts. For example, the last clause of the verse, 'Because strait is the gate, and narrow is the way, which leadeth unto life; and few there be that find it,' is very frequently interpreted as meaning 'some do find it.'

In this connection there is another not uncommon error, namely, that of regarding two propositions, worded differently, as different assertions, when, in fact, they assert the same thing.

But traditional logic is of little help in the whole matter of logical analysis. For instance, its treatment of conversion makes dry reading and a perfunctory task for the student. It is even worse; it savors of the artificial and useless. That a subject of such importance as conversion should be presented in a way so unnatural and forbidding as that of the traditional logic is much to be regretted. Logic should teach in this matter, not traditional rules, nor discussions of formal subtleties, but the simple truth that the rightness or wrongness of every converse rests on precisely the same basis as that of the original proposition, namely, known facts and laws. The proposition that ignorant people are superstitious is true because it agrees with the facts. But if we change it into superstitious people are ignorant we do not get a good converse, because this proposition does not agree with the facts.

In the next place, the syllogism needs more radical change in treatment than either of the two previously mentioned divisions of so-called deductive logic. The traditional treatment has overloaded the subject with dry discussions, rules and symbolical schemes, so that there is hardly left the slightest appearance of any connection with actual thinking. Better omit all mention of figure, mood, reduction, and the question whether there are three or four figures, than miss the important lesson of the syllogism. "There is little," says Mr. Sidgwick,* "that need be taught about the syllogism, since the process itself —which is merely that of bringing a particular case under a general rule—is used instinctively by every one from childhood onwards." Examples like these, 'Five francs are a dollar, four shillings are a dollar; therefore five francs are four shillings'; or, 'Some men are not fools, yet all men are fallible,' are not suited to bring out the real 'process,' much less to train the mind in accurately applying a general truth to a particular case. In fact, too many of the arguments selected

* Sidgwick, 'Use of Words in Reasoning,' p. 354.

by authors of text-books for illustration and training in syllogistic reasoning serve little more than to show the machinery of the syllogism. Others are either too hackneyed, transmitted as they have been from one generation of writers to another, or are lifeless fragments and consequently mean scarcely more than so many words. Then, again, cases of faulty argument constitute too large a proportion of text-book exercises. If selected with discretion, defective reasoning may be used to bring out in the most emphatic manner certain mistakes commonly made in thinking. If, however, examples of bad reasoning are too patently wrong, or if they appeal simply to the instinct of curiosity and afford an excess of amusement, they are likely to fail in elucidating principles and establishing correct habits of thought. On the whole, fresh arguments taken from living thought, which are also models of accurate thinking, should be more extensively used.

At this point in the teaching of logic comes the real test of the instructor's skill. Instead of relying upon the text-book, he must depend for illustration upon his own resources. And, as far as possible, the illustrations should be presented in their full form, as actual arguments, and not in the condensed and lifeless way that text-books from lack of space are forced to do. Moreover, what is of even more consequence, he must be able to stimulate students to find material for themselves. There is no more direct and practical way than this for the student to cultivate the critical habit of mind in reading newspapers and periodical literature, as well as the literature of the various subjects of his college course. Such material would constitute what John Morley has somewhere called 'reasoning in real matter.' "It would make such a manual as no other matter could, for opening plain men's eyes to the logical pitfalls among which they go stumbling and crashing when they think they are disputing like Socrates or reasoning like Newton. They would see how a proposition or an expression that looks straightforward and unmistakable is yet, on examination, found to be capable of having several distinct interpretations and meaning several distinct things."

Of course there is danger of using exercises that for one reason or another are beyond the grasp of the sophomore or even the average senior. And yet, I am inclined to think that whatever pedagogical mistakes have been made in this connection have for the most part been in the direction of making the illustrations and problems so commonplace and simple as to seem silly. Indeed there is much to justify the student in real life in making the criticism of the student in the story, who says: "When they spring those tricks on you about the flying arrow not moving, and all the rest, and prove it by pure logic, you learn what pure logic amounts to when it cuts loose from common sense."

It is more evident in the ordinary treatment of inductive logic than in the case of deductive, that the subject is descriptive in character, with its data taken from the work of scientific investigators and discoverers, and its purpose to set forth the approved ways of thinking. Nevertheless, more might be done to anticipate a first impression, not at all uncommon with students, that the subject matter of inductive logic is abstract and quite removed from daily human interests. There is no danger of over-emphasizing the relation of what is taught in the class room to the realities of life, by way of showing that the content of logic is not the invention of text-book writers, and is not esoteric in its nature and use; that the methods described and analyzed in treatises on logic may be said to have their primitive forms in the uncultivated state of the human mind whether in savage or in civilized society. What is scientifically known as the uniformity of nature and the method of difference are but the tendencies of the human mind to expect similar coexistences and sequences under similar conditions, and to regard the new antecedent as the cause of the new phenomenon, tendencies as strong in the savage as in the civilized mind. In brief, we should show the student that the difference between the principles and methods of common life, and those studied in logic, is the difference between spontaneous and attentive observation; between rash and rationally guided theorizing; between verification that is heedless and insufficient, and that which is exact and exhaustive.

Induction should be understood in its proper connotation. To conceive it as simply the reverse process of deduction, to regard it as identical with case-counting, or mere generalization based upon facts, is to remain ignorant of the complex and varied nature of scientific method, in which generalization plays but one part. A logical analysis of inductive method should be so complete as to make it unmistakably evident that to be a scientific investigator is to be more than a collector of facts and a propounder of theories. Darwin once remarked that any fool could generalize and speculate. The verification of theories by appeal to facts and known laws is the step in inductive procedure quite frequently overlooked or hastily taken. And yet the importance of it is emphasized by what has been said of eminent scientific investigators, namely, that the process of deduction has played a more important part in their work than induction; that their days were spent in verifying their theories and establishing the further consequences of them.

The failure to understand the complex nature of inductive method appears now and then in another form. It finds expression in the opinions of those who profess to speak authoritatively upon the study of science from the pedagogical point of view. According to this view, the distinction between the observational and the experimental

sciences is of insignificant value, and the arrangement of science courses might well enough be determined by local and economic conditions.

This misconception should be classed with that of mistaking general for singular terms, as is often done in the case of moral law and natural law. For it regards science as all one and the same, having one invariable procedure in all branches of scientific research, regardless of the peculiar nature of any particular group of phenomena. Consequently a study of any one of the sciences ought to satisfy the modern demand for science study and should qualify the more brilliant students as competent and reliable investigators in any branch of science whatsoever. If this were true, then, so far as pertains to method, the chemist might at once turn psychologist and pursue his work as successfully as though he had received his training in psychology instead of chemistry. The ideal man of science would be the last person to make any such claim. For he well knows that, besides the common features of the scientific method which appear everywhere in their broad outlines, there are numerous variations due to individual characteristics possessed by the data of the various sciences or different groups of sciences, and that to be a good scientist requires a preparation in the field in which one is to work. A better understanding of these facts might do much towards dispelling illusions as to a model science and the superiority of one science over another. Unfortunately scientists often assume an unscientific attitude towards one another. The physicist, for example, declares that for one to undertake the scientific study of psychical phenomena is to sound his death knell as a scientist. There is much need, among investigators in the various fields of human interest, of increased respect for one another's methods and results; of an intelligent conception of the peculiar conditions and difficulties of problems other than one's own; and instead of ridicule and depreciation, a just and cordial recognition of contributions honestly made, even though they lack the precision and finality which characterize results obtained elsewhere.

In addition to an orderly presentation of scientific methods and analysis of important and interesting conceptions such as the uniformity of nature, cause, hypotheses, theory, law, inductive logic should make it very evident that the data of our thinking are varied, and that the character of many conclusions is problematical. The facts of human experience, the problems of the world at large, do not lend themselves to any 'secure method' or yield conclusions that are certain. At one time we must act decisively on inferences which are far from approximating to certainty; and then again when it is not a question of choosing or starving, we need that suspended judgment which has been called the greatest triumph of intellectual discipline. In brief,

a course in minor logic constructed along the following lines will, to my mind, render the best educational service: Definition and classification, with special emphasis upon *use* as determining the meaning of terms. The interpretation of propositions and the relation between form and meaning, with much stress laid upon the complexity of actual thinking rather than upon categories, predicables and symbols. The study of the syllogism as a form to which arguments may be reduced for the purpose of critical analysis. Training in ability to examine the validity of premises and their application to particular cases. And finally, the study of inductive methods with the view of familiarizing students with the actual ways by which knowledge is discovered.

All this means that logic is essentially a psychological rather than a philosophical study; that its content is mental phenomena, because the study of methods is but the study of the human mind engaged in the search of truth; that induction and deduction are in reality two constantly interplaying processes and are never found to be what the time-honored division of text-books suggests. Discussions of controversial topics, in which logicians delight, and from which no text-book, so far as I know, is wholly free, should be excluded. They have little interest for most students and besides obscuring the real content of minor logic, are likely to produce the impression that logic lacks definiteness and substantial basis. It is much better to hand over speculative questions to philosophy proper. Enough will be left for the course in logic in the time usually allotted it. What Professor Hyslop has said is eminently true: "Logic has been made too formal for usefulness and postponed too late in the course. It ought to follow mathematics immediately, to correct the confidence in reasoning that that science inculcates consciously or unconsciously."*

A word, in closing, upon a possible criticism of that part of logic which treats of inductive method. Why study logic in order to become familiar with the methods of science? Why not go directly to the several sciences themselves?

"We sometimes can not see the wood for the trees; and lose the broad outlines in the multiplicity of details." Just as we need to get out from among the trees to look at the wood; to stand some distance from the building to get a full view of it, so the scientist must needs view the structure of scientific knowledge from outside his own special field.

It is a frequent experience that students become so engaged in the multiplicity of fascinating phenomena of one science, or charmed by

* *The Psychological Review*, 1903, p. 180. Sidgwick's 'Use of Words in Reasoning,' and Aikins' 'The Principles of Logic,' show an encouraging tendency away from the traditional treatment of logic.

mechanical manipulations, that they are oblivious to underlying truths shared in common with their own and other subjects, or perhaps fail to appreciate the individual characteristics of a particular subject or group of allied subjects.

It can not be gainsaid that "a scientific habit of mind can be acquired only by the methodical study of some part at least of what the human race has come scientifically to know." But logic may supplement this indispensable kind of training. In it the methods themselves are made the direct objects of study. Brought together from near and far they may be compared, analyzed and classified with the attention focused upon them in their broad outlines. So presented, with a good body of illustrations, they may be above criticism as too formal or abstract, and furnish both layman and specialist with means of cultivating the sense of discrimination and widening their interests and sympathies.

THE MUTATIONS OF LYCOPERSICUM.

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DURING the years 1901 to 1903 inclusive I published results of my observations and experiments concerning the horticultural variability, atavic reversion or degeneration, and phylogenetic mutation of the common cultivated tomato. The reader is referred to those publications for such statements of pertinent facts as may be omitted from this one.* The object of the present article is to give in popular form a concise restatement of my experimental observations upon some remarkable cases of saltatory plant mutation and varietal changes of the tomato fruit, together with figures and additional discussions. Although the cases of plant mutation referred to constitute the leading part of my subject, I will first discuss the origination and and decadent extinction of the improved fruit varieties which have arisen in connection with, and apparently as a result of, horticultural conservation. These discussions are necessary to the making of a clear distinction between fruit variation and plant mutation as I shall have occasion to refer to them.

The enormous increase in the importance of the tomato as an article of food during the past thirty years has so stimulated its cultivation that very many fruit varieties of fine quality have resulted, figures and descriptions of the more important of which are annually published in seed growers' catalogues. During that time also at least two new specific plant forms have suddenly originated by mutation from the common species,† *Lycopersicum esculentum,* making not less than three species of the cultivated tomato. It is desirable to characterize these species briefly in connection with the discussion of the fruit varieties which they bear. The two new species referred to I have called *L. solanopsis* and *L. latifoliatum,* respectively, of course leaving the original name, *L. esculentum,* with the unmutated, or mother form.

* 'Varietal Mutation in the Tomato,' *Science* (n. s.), vol. xiv., pp. 841–844, New York, Nov. 29, 1901. 'The Saltatory Origin of Species,' *Bull. Torrey Bot. Club,* vol. xxix., pp. 511–522, New York, Aug., 1902. 'My Tomato Experiments,' *The Independent,* vol. liv., pp. 2460–2464, New York, Oct. 16, 1902. 'Aggregate Atavic Mutation of the Tomato,' *Science* (n. s.), vol. xvii., pp. 76–78, New York, Jan. 9, 1903.

† To avoid undue repetition, the terms 'species' and 'plant forms' are used interchangeably; and the term 'mutation' is used in its now accepted sense of sudden origination of species.

This mother form, even as delimited by the mutations just mentioned, is much the most common one in cultivation. Its early stage of growth is fairly represented by the accompanying Fig. 2. The form to which the name *L. solanopsis* is given is well represented in a similar stage of growth by Fig. 1. Short descriptions of these two forms are recorded on a following page. I am not now able to present a figure of *L. latifoliatum*, but it is represented by the plant which bears the fruit variety known to gardeners as the Mikado, or Turner's hybrid. I think the latter name, when applied to the plant, is misleading because this specific plant form doubtless originated by true mutation, as *L. solanopsis* has done; and it is by no means certain that even the fruit variety which it then bore originated by hybridization.

Fig. 1. Representing *L. solanopsis*, the Daughter Form.

Fig. 2. Representing *L. esculentum*, the Mother Form.

The difference of *L. latifoliatum* from the two other forms mentioned is conspicuously seen in its peculiar foliage, the leaves having decurrent petioles and broad, flattened leaflets with their borders entire instead of notched or crenulated. These three species are as well defined and distinct as are any others of the dozen recognized species of *Lycopersicum*, and as distinct as are many of the recognized species of other plants, whether wild or cultivated, and there is apparently no tendency of the two derived species to revert to the mother form. I do not know of any case of hybridity between any two of them, and no indication of further mutation of the two new species has been observed.

It is from these three specific plant forms that the improved fruit varieties have arisen. The greater part of them have arisen from *L. esculentum*, as that species has been delimited; a considerable number have arisen from *L. solanopsis*, while *L. latifoliatum* has hitherto shown the minimum of varietal change in its fruit. These varieties

differ more or less from one another in shades of color, but their valuable qualities appear in the increased solidity or consistence, size and fine flavor of the fruit. Their origination has, in many cases at least, been sudden and fortuitous; and, although their specific characters are usually heritable, they are so peculiarly subject to degeneration that they hardly possess racial properties. The causes of their origination and extinction appear to be irrelative, and both are independent of specific plant mutation. Still, as I shall show, a few cases of coincidence of their origin with plant mutation are known; and more or less of common plant variation, and even of dwarfing, have sometimes occurred coincidently. Moreover, all cases of atavic reversion, or degeneration, of fruit varieties from their fine quality have apparently been unaccompanied by any material change in the plants bearing the degenerating fruit. Frequently special reference to those reversions will be made further on, but I will first turn aside for a few more or less theoretical remarks upon the character and manner of origination and extinction of the fruit varieties.

In the simple cases of origination of the fruit varieties of the tomato, unaccompanied with plant mutation, the change occurs only in the pericarp, which becomes the fruit. Also, when atavic reversion occurs it is only the pericarp, that is, the fruit, which is materially affected; and both kinds of these changes are doubtless of molecular origin in the germ cells. Likewise, in all cases of phylogenetic plant mutation the initial act is molecular, and occurs in the germ cell of each ovule which gives origin to a new plant form.* There is no apparent reason to doubt that the mutative acts which thus respectively produced the two new specific forms from *L. esculentum* might have occurred without the coincident production of a new fruit variety, but as a matter of fact a new fruit variety of high quality was coincidently produced in each case; and the coming change in both plant and fruit was doubtless initiated in one and the same germ cell. Still, because of the essential difference between plant mutation and fruit variation, I think this pericarpal change was only a varietal coincidence, and not an essential part of the phylogenetic process in those cases. Moreover, that change is known to have occurred separately in other cases, and to have resulted only in weakly heritable fruit varieties.

Known cases of degeneration, and final extinction of fine fruit varieties as such, are somewhat numerous, for their instability is very great, although at least most of them remain true to seed from year to year under favorable conditions. Whether the varieties which have

* For exhaustive discussions of this and kindred subjects see 'Intracellulare Pangenesis' by Hugo de Vries. Jena, 1889; and also 'Die Mutationstheorie,' vol. 1, by the same author.

arisen from the two new specific plant forms that were derived by mutation from *L. esculentum* are more permanent than are those of the mother species I do not now know, but all the cases of atavic reversion known to me have occurred with fruit varieties of the mother species. It therefore seems possible that the fruit varieties arising from *L. solanopsis* and *L. latifoliatum* may be less liable to hybridity, or otherwise more permanently heritable, than are those arising from the mother species, *L. esculentum*. The fruit of the two derived species has always been of fine quality and, for them, intraspecific fruit reversion would not be degeneration; but the original fruit of the mother species was very inferior, and any reversion of its improved quality would be degeneration. For those who may have the opportunity, it will be interesting to observe the relative permanence of the fruit varieties arising from the different species, and the course that may be taken in any qualitative changes that may occur in the fine fruit varieties of the two derived species. In the case of the mother species the trend of fruit degeneration is direct, intraspecific and towards the primitive fruit condition. If similar reversional trend in the fruit of the two derived species could occur it would necessarily be accompanied by coincident reversion of specific plant characters, an occurrence which I think improbable, or we should have one and the same kind of degraded fruit borne by different species.

Returning to the practical consideration of atavic reversions of fine fruit varieties, it may first be mentioned that they often occur locally and affect only a few plants, or the crop of a single garden or field, the variety thus affected remaining unchanged elsewhere, but, as I shall show, cases of reversion are often much more extensive. The progress of reversion is sometimes slow and sometimes sudden, the whole change in the latter case often occurring in a single generation. The effect is much the same whether the degenerating process is sudden or slow; and however widely the improved varieties may have differed from one another, the reversional trend of all is towards the comparatively small globular berry that may be properly regarded as the primitive tomato fruit form. In slow degeneration the fruit begins to ripen unevenly; it diminishes in size and becomes comparatively soft, and has a rank taste. The walls and dissepiments become thin, the seed compartments are reduced to four, three, and even to two, and the seed pulp is more abundant and more watery. In sudden reversion the primitive berry condition is reached, or approached, at a bound. These remarks concerning degeneration apply especially to the varieties which have arisen from *L. esculentum*, as delimited, of which cases only I have had personal knowledge.

With the possible exceptions which were merely suggested in a previous paragraph, the duration unimpaired of any of the highly

improved fruit varieties of the tomato is only a few years under favorable conditions; and constant care is necessary to maintain their fine quality. Therefore the tendency to deteriorate is doubtless inherent; but this result is evidently hastened by careless cultivation, repeated planting of the same ground without rotation with other crops, the growing of plants from unharvested seed, cross pollination with inferior kinds, differences in character and fertility of the soil, and the influence of a climate much warmer than, or otherwise different from, the one in which the seed was produced. That this fruit degeneration is sometimes slow and sometimes sudden; that it is imminent and variously excited to action; that it is not confined to sporadic cases of single plants, but may, and often does, equally affect a whole crop, and sometimes all the crops of a wide region, is shown by the following statements of relevant facts.

Every person who habitually visits the vegetable markets of any one of most of our towns and cities which are supplied from neighboring gardens is familiar with the different grades in quality of the tomatoes there on sale. Indeed, it is often easy to recognize among them different stages of reversion from some of the more common improved varieties, notably the Acme. These are too plainly cases of gradual degeneration, resulting from careless cultivation and crossing with inferior kinds, to need explanation. Several of my correspondents have furnished me with important corroborative facts. Dr. Geo. G. Groff writes that he has for many years observed in central Pennsylvania, that tomato plants which sprang from seed of good varieties left in the ground during the winter always produced inferior fruit, usually the small kind called cherry tomatoes. Miss Mary E. Starr informs me that during her residence in Saint Martin's Parish, southern Louisiana, her father found it necessary to procure tomato seed from the north for every crop grown on his plantation, because the seed from even the first crop of tomatoes grown there usually produced very small and inferior fruit. Mr. L. S. Frierson, however, writing from northwestern Louisiana, says that he has produced excellent fruit, true to seed, from his home-grown crops. Mr. H. J. Browne, of Washington, D. C., sent me from a plantation near Havana, Cuba, a small parcel of cherry tomatoes taken from plants which he found growing there luxuriantly. The planter assured him that they were the immediate progeny of the first Cuban crop of a fine large fruited variety, the seed of which he obtained from New York under the well-known varietal name of Trophy. He also asserted that such degeneration was always the result of his attempts to raise tomatoes from Cuban-grown seed, however fine might be the variety from which his original seed was obtained. The fruit of the first Cuban crop, like that of southern Louisiana, was always true to northern seed,

showing that the initial step towards atavic change took place in the germ cell of every one of the first seeds produced on those southern soils, and that the reversion was therefore sudden and aggregate. Mr. Browne, who has business interests in Calapach island, which lies thirty miles east of the Isle of Pines and eighty miles south of Cuba, also informs me that there are now growing on that island tomato plants which are four or five years old, they having changed from the condition of annual, to that of perennial plants in that tropical climate. Furthermore, the fruit of those plants has changed from a good variety of large size for the first fruitage to the cherry form and size before mentioned for the later fruitages.

These credible facts, gathered from widely different sources, plainly indicate that various exciting causes of varietal fruit degeneration exist, but they throw little light upon the real nature of those causes. The facts mentioned also indicate that many new opportunities are likely to arise for scientific agricultural experimentation with the tomato. Our tropical and subtropical island possessions will doubtless soon be called upon to supply, for our own and other countries, the increasing demand for early tomatoes, just as northern Egypt has been made the early tomato garden of Europe. My present object in referring to these facts, however, is their application to the second part of my subject.

This second part pertains to phylogenetic plant mutation as distinguished from ordinary plant variation and the production of new fruit varieties. The immediately following remarks embrace in narrative form an account of two cases of saltatory plant mutation which have fallen under my experimental observation. In the spring of 1898 I purchased a couple of dozen young tomato plants of the Acme variety which had been germinated by a gardener near Washington, D. C., and transplanted them, before any of their flower buds were formed, in a garden plot of a few hundred square feet, upon my house lot in the city. Short specific descriptions of these plants and their progeny are given for the purpose of showing their differences.

As the plants matured and fruited they were found to possess all the characteristics of the Acme variety, and of typical *L. esculentum*. They early became decumbent, and at full maturity they were large and diffuse; the haulms, which were slender and somewhat numerous, reaching a maximum length of more than two meters; color of the foliage a comparatively light green; the petiole-midribs long and slender; leaflets moderately narrow, distant, petiolulate, and their surfaces only slightly rugose; fruit of moderate size, usually depressed-globular in shape, but sometimes transversely oval, uniformly ripened, fleshy and well flavored, and in ripening the chlorophyl green changed to a deep crimson through more or less of yellow.

I selected seed from the fruit of the best plants of this crop of 1898, cured, and sealed them in a packet, and planted a random portion of them in my garden in 1899. I expected to produce true Acme plants from this sowing because of the well-known comparative stability of that variety and of my care in selecting and preserving the seed; and also because no other tomato plants were grown with them or in the same neighborhood, from which cross fertilization might have occurred. To my surprise, however, all the plants which grew from those seeds were distinctly different from the parent plants, not only as to fruit, but as to specific details of plant structure; and they were all alike in those characteristics. It may be incidentally mentioned that a difference was recognizable in the earliest stage of growth of the plantlets; the cotyledons were proportionately short, placed low on the stem, and in a goodly number of instances, triple; a character which I have never observed in any other tomato plantlets. At maturity the plants were sturdy and compact, standing erect with little support until after the first fruits were visible, and reaching a mature length of only about two thirds of that of the parent plants; haulms strong and comparatively few; foliage dark green; petiole-midribs short and strong; leaflets moderately broad, not distant, sessile or nearly so, and their surfaces strongly rugose; fruit similar to that of the parent plants in size, shape and consistence, but of finer flavor and more delicate in color, changing from a dark chlorophyl green to cherry red or light crimson through a neutral or flesh color, and not through yellow. I preserved no seed from this crop of 1899, and supposed the fruit variety was therefore lost, as indeed it was, but two years later I recovered it, as will presently be shown. This fruit differed considerably from any other of the numerous varieties known to me; but the plants had essentially the same specific characters as those which had previously been produced by gardeners, known as the potato-leafed variety of tomato. It was by those characters that I designated *L. solanopsis* as a distinct species.

In the spring of 1900 I purchased from a Philadelphia company of seed growers a packet of their 'selected Acme tomato seed,' which was grown in 1899 on a Pennsylvania farm, more than a hundred miles from the place where my first Acme plants were grown. From this seed I grew thirty plants to maturity, every one of which, with its fruit, was true to the Acme variety as I have just described it for my crop of 1898. Fig. 2 represents one of those plants as it appeared in the early stage of its growth. Its smaller size than Fig. 1 is due only to the relative size of the growing plants at the time the photographs were taken. The conditions of cultivation in this case were identical with those in the former case; no other tomato plants were grown with them, nor were any grown in the neighborhood; and the

resulting crop of fruit gave no visible indication of impending mutation. I as carefully preserved seed from this crop as I had done in the former case, and planted them in my garden in 1901, believing that I should produce Acme plants, notwithstanding my former experience. On the contrary, the result was an exact duplication of my experience with the crop of 1899, every plant and every fruit partaking fully and uniformly of the duplicated mutation. The plant description, including that of the fruit, which is given in the immediately preceding paragraph applies as exactly to the plants of the crop of 1901 as it does to those of the crop of 1899. Fig. 1 represents a plant of this crop in the early stage of its growth, when it was beginning to shed its first flowers. Its deeper shade of green adds to the difference of aspect between the mother and daughter forms.

The Figures 1 and 2 are copies of photographs which were taken of the plants as they were then growing in my garden.* The plant represented by Fig. 1 bore the new variety, which I have called the Washington. That which is represented by Fig. 2 bore the Acme variety. Although it can not be proved that the particular plant which is represented by Fig. 1 actually came from a seed borne by the plant represented by Fig. 2, I do not hesitate to assert positively that the plant form represented by Fig. 1 is the immediate progeny of the form represented by Fig. 2. I make this statement with all the more confidence because all the work of my garden has been done *con amore* by my own hands, including the planting of the seed, the plucking of the fruit from which the seed was taken, and the curing and preserving of the seed for the next year's planting. In all this work I practised the same conscientious care that I have done in all my other scientific work in other fields. No tomato seed other than that which I have mentioned was in my possession during all the time my experiments were in progress, and I do not admit the possibility that any other seed was at any time substituted. Even if there had been any such substitution, it would not account for the mutations which I have described, which were phylogenetic in character and not the result of hybridization. The fruit of the mutated plant species was also a new variety and would not, in any ordinary case of germination, have been produced by seed of any other variety previously known. This new variety is as distinct as are any other fine varieties, and it has been true to seed every year since its origination. If my Acme plants, in either of the cases mentioned, had received adventitious fertilization by pollen from any other flowers than those of their connate crop associates, the cross fertilization would

* These figures were originally published in an article by Dr. R. Francé, in *Die Umschau*, at Frankfurt am Main. In that article it was unfortunately stated that Fig. 1 represents the mother form, while the reverse is the fact.

certainly have been incomplete as to the whole crop and various as to the kinds of hybrids produced. Even if it were credible that the first case of complete aggregate mutation was due to fortuitous cross fertilization from some unknown source, it would still be too much to believe that exactly the same hybridizing process should have been repeated in the same manner in a following year. It may be added that there is now much reason to doubt that hybridization, although always imminent among tomatoes, has ever been so effective an agent in producing improved varieties of either plants or fruit as has been generally believed. Indeed, saltatory mutation and racial variation have doubtless produced many of the results among plants that have been attributed to hybridization; although the latter has produced many wonderful results.

At the close of this narrative of experimental observations it is well to call special attention to the assumed fact that the mutative process which produced the new plant form that has been described was essentially separate from the accompanying process of fruit variation, although the two processes were intimately associated in both their origin and development. The plant mutation was from *L. esculentum* to *L. solanopsis;* the fruit variation was from the Acme to the Washington variety. The new fruit variety which accompanied the new plant form is of fine quality and therefore of horticultural value; but the origination of any fruit variety is, from a naturalist's point of view, of far less importance than the origination of a species. Plant mutation produces species which are real entities. Fruit variation is limited to changes in the pericarp; and the most improved and heritable fruit variety thus produced may, by degeneration, become disassociated from the plant entity without any impairment of that specific condition.

There are two extraordinary features of the foregoing narrative of my observations. One of them relates to the sudden and complete mutation of every plant of a crop of twenty-four Acme tomato plants to another specific plant form bearing a new variety of fruit. The other relates to a subsequent exact duplication of that mutation in all its details as to both plant and fruit, in a crop of thirty plants, also of the Acme variety. It is apparent that both cases were initiated and consummated in the plants while they were growing in my garden because the germ cells which gave origin to the mutated plants were all formed there; and the mutated plants were there grown to maturity. Another fact, important in this connection, although stated in a previous paragraph, is that this new specific form had been previously produced by gardeners, who had given to it the name of potato-leafed tomato. That is, this one and the same species, *L. solanopsis,* has arisen suddenly and independently from *L. esculentum* at not less than three different times, each in a different locality.

Extraordinary as were the two cases of aggregate phylogenetic plant mutation which came under my observation, they are no more wonderful as natural phenomena than are the numerous cases of sudden and aggregate atavic reversion of previously constant and heritable fruit characters which have been mentioned in this and other publications. Indeed, among tomatoes, the aggregate occurrence of both plant mutation and atavic fruit reversion appears to be quite as normal as does their separate or individual occurrence. In both kinds of these cases, although their results are so different, the initial change has occurred in the germ cell of each of the seed ovules which gave origin to the affected plants. Both kinds are of mysterious, but doubtless natural, origin. Still, I can make no suggestion as to what may be the nature of either the determinate, predisposing or exciting cause in any of these cases.

It is not necessary, but it may not be inappropriate, to say that the foregoing paragraphs have not been written from a biometrical point of view, but from that of an old time naturalist. The principal facts which are there recorded have presented themselves to me with more force than I feel able to present them to others. I am still greatly impressed with their remarkable character, especially because they are not in accord with my own former views. Some of them also are known to be at variance with commonly accepted views of horticulturists, but I present them all with full confidence in their accuracy. Indeed, I do not admit the possible occurrence of any error that could have been instrumental in producing any of the phenomena which are here recorded.

Notwithstanding the peculiar features of these two cases of sudden mutation in the genus *Lycopersicum*, I assume that in their essential nature they are to be classed with those cases of mutation which have been observed in the genus *Œnothera* by Professor de Vries, and which he has used in demonstrating his theory of mutation.* In his experiments with those plants, popularly called evening primroses, he repeatedly observed, in different years, the origination by sudden mutation of a few individual plants of one and the same species among the numerous progeny of the mother species. He also observed the similarly sudden and rare mutation of several new species from a mother species, but he has not reported any case of mutation of all the progeny of any one plant of a mother species; much less the progeny of a whole crop of plants, such as I have observed with reference to the genus *Lycopersicum*.

* See 'Die Mutationstheorie,' von Hugo de Vries, Volumes 1 and 2, Leipzig, 1901, 1902; 'The Mutation Theory of Professor de Vries,' by Charles A. White, Smithsonian Report for 1901, pp. 631–640; and 'A New Theory of the Origin of Species,' by A. Dastre, Smithsonian Report for 1903, pp. 507–517.

The number of specific mutations which were observed in *Œnothera* by Professor de Vries was greater than the number that has yet been observed in *Lycopersicum;* but the scope of mutative action in *Œnothera* embraced only a very small percentage of the abundant progeny of the mother plants; while in the two cases of mutation in *Lycopersicum* which I observed, that action embraced all the progeny of a small crop of mother plants. The mutative period in *Œnothera* occurred as a correlative of the extreme activity of natural reproductiveness and geographical distribution, but that period occurred in *Lycopersicum* as a correlative intensive cultivation. The unusual conditions, although so different in each case, apparently made the mutative opportunity available for the respective species. Other conditions will doubtless be found to give other species that opportunity, with diverse results. When other plants shall have been discovered in their mutative period the scope and diversity of mutative action will probably be found to differ in each case. If so, no one case can be made the absolute standard for such action.

The observations of Professor de Vries, as well as my own, show conclusively, not only that species may originate by sudden mutation, but that one and the same species may thus originate independently at different times and places and from different plants of a mother species. This fact is not without obvious significance in connection with geographical distribution of living species and the origination and distribution of organic forms during geological time.

BIOLOGY IN THE ROCKY MOUNTAINS.

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FOR the purposes of this article the term Rocky Mountains will be understood to mean the states and territory including and surrounding these mountains in the United States; that is to say, New Mexico, Colorado, Wyoming and Montana. The area thus indicated, stretching from north to south, and including both mountains and plains, is of course extraordinarily varied. Because of the different conditions of temperature, moisture, soil, etc., found within its borders, it possesses as a whole a fauna and flora extremely rich in species. It offers, in the high mountains and to the north, a large assemblage of circumpolar types, some exactly like those of northern Europe and Asia, others variously modified. It gives us, on the plains and to the south, a series of species of Austral origin, some of them intruders even from the tropics. Still again, in its valleys and forests, it has developed a large number of endemic types, found nowhere else in the world.

Such a region necessarily presents great attractions to the naturalist. It has been visited by numerous government expeditions and private individuals, beginning early in the nineteenth century, for the purpose of collecting its scientific treasures. It has yielded to these an abundant harvest, not only of living animals and plants, but also of fossil forms. Every museum of any consequence contains Rocky Mountain material, and innumerable publications are devoted to its description and illustration. These being the facts, a superficial observer might very well conclude that the natural history of the Rocky Mountains was thoroughly known. So far, however, is this from being true that it would be more correct to say that the scientific study of Rocky Mountain biology has hardly begun.

Any one who examines the published accounts of Rocky Mountain animals and plants will find, at least in the majority of groups, little more than descriptions of species. Putting aside the enormous number of species still undescribed, we find that the 'known' species are in fact very little known at all. Among the insects, for instance, there are hundreds of which we do not even know the locality, nearer than the name of the state, and those of which we know the life history are comparatively few. The details of geographical distribution, the char-

* Since this paper was written, the writer has moved to the University of Colorado, at Boulder. Mr. L. C. Himebaugh is now in charge of Colorado College Museum.

acter and extent of the variations, the interrelations with other species —these are things rarely mentioned in accounts of species described from our area.

The reason for this state of affairs is evident enough. Nearly everything has been done, until quite recently, by naturalists who resided in the eastern states. It is true that many of them visited the west, but usually to hurriedly gather together such miscellanea as came in their way, to take them home and there study them at their leisure, or turn them over to the appropriate specialists. Very little was done on the ground, except by a few resident naturalists, who were usually at a disadvantage because of the absence of libraries and museums. Even to this day, one comes across that deep reluctance to form independent conclusions, born of the feeling that in biology, as in other things, the wise men of the east hold the keys of knowledge. It is exactly the attitude which Americans of the eastern states, a hundred years ago, used to show to the naturalists of Europe.

Eastern science is the mother of that of the west, and European science is its grandmother. May this relationship never be forgotten; but the time must come when the young fellow will stand for himself. I think and hope that this time is rapidly approaching, and therefore regard with more than ordinary interest the new developments in the educational institutions, which begin (only begin!) to make intellectual independence in biology a possibility.

Such talk as this is not mere bombast; such wishes are not merely born of that mania for supremacy which afflicts so many peoples. We do not wish to do any more than look after our own affairs, and that we surely are entitled to do. The point is that, after all, biology is the study of living things, and the descriptions of museum specimens are only preliminary to the most important part of the work. It is utterly impossible that the innumerable problems raised by different aspects of our fauna and flora (biota, let us say, after Stejneger) can ever be solved except on the ground. And our eastern friends—they have their own region, very far from being exhausted, besides having to look after material from all sorts of countries where there are no resident naturalists, or very few, and adequate facilities are not even in prospect.

I am not proposing a sort of Monroe Doctrine in biology. Professor Underwood, not very long ago, did advocate something of this kind; proposing that Europeans should attend to their own flora, or at least to that of their own hemisphere, while Americans looked after American plants. This, if I understood it rightly (and it was plainly put!), was not a very defensible proposition; for imagine the results of the two halves of the circumpolar flora being studied entirely apart! Indeed, one has only to examine existing publications to see numerous ill results of this provincialism—and it is provincialism, though one's province be as large as the two Americas. So far from wishing to

isolate Rocky Mountain naturalists and their work from that of others, I wish exactly the reverse. The time will come, I think, when no single man will think of producing a monographic work on a group of organisms. He will compile the work, adding to his own contributions those of others from every region inhabited by his chosen beasts. In that day the local naturalist will contribute his part; but the point is, he will make his own observations, and will not merely send material for the all-wise one to ponder over.

The history of Rocky Mountain mammalogy is quite interesting. During the nineteenth century 68 new mammals were described from our area. Of these, six are not now considered distinct, but 62 remain. In the first decade, two were described by Ord. In the twenties, Say made known five, in the thirties Bachman described two, in the forties nothing was added, in the fifties we have six by Baird and one by Audubon and Bachman, in the sixties two by Kennicott and one by Hayden, in the seventies one by Coues, in the eighties one by Shufeldt and one by Merriam. Thus, to the end of the eighties, 22 had been described. Now in the nineties, counting 1900, no less than forty were added, mostly by Merriam and Allen! In 1901 three more were added, and in 1902 five. I first came to Colorado in 1887, and remember very well having the distinct impression that the species and subspecies of Rocky Mountain mammals were very well known. This, indeed, was the accepted view; but how wonderful was the result of assiduous collecting and study during the next ten or twelve years! It is admitted that not all of the newly named animals are very distinct, but some are, and all appear to have their characters.

Of all these descriptions, one was the joint work of two resident naturalists, but the rest were prepared by students living in the east. Perhaps one should make a second exception of the mouse described by Dr. Shufeldt, who resided for a considerable period in New Mexico. The number of new forms described from Colorado, New Mexico and Wyoming is about the same, Colorado being a little in the lead; but only seven, less than half the number of the other states, come from Montana. The northern state, however, can pride itself upon containing the type locality of the grizzly bear; this and the common wood rat (*Neotoma cinerea*), also from Montana, being the two first-described animals from our region.

It is not necessary to similarly outline the history of other groups, but it may be said that the flowering plants are in the midst of a revival period quite equaling that of the mammals, while the description of new insects goes on at a very rapid rate. Of over 500 wild bees collected in the last ten years or so in New Mexico, more than 300 have been described as new.

In order that it may be understood that something is really doing in the Rocky Mountains, I propose to briefly describe the existing

facilities for work and say a little about some of the workers. I begin with Colorado Springs, merely because it is near at hand. We have in this town a few good naturalists. The senior member of the fraternity is Mr. Aiken, after whom the snowbird *Junco aikeni* was named. Mr. Aiken has, I suppose, the best collection of birds in this part of the country, and what is more to the purpose, has a really critical knowledge of them. A few days ago, I had the pleasure of reviewing with him a woodpecker which he believes to be new, and I hope he will publish an account of this and other interesting birds which he has studied. Our other bird man, Mr. Edward R. Warren, is also interested in mammals, and is making a remarkably interesting collection of small mammals. Mr. Warren is much interested in the photography of living wild animals and birds; and some of his photographs, especially those showing the ptarmigan in all plumages, are exceedingly beautiful. Professor Cragin, the well-known paleontologist and zoologist, is resident here, but now devotes himself entirely to the history of the west. The types of most of his new species of fossils are in the museum of Colorado College. Professor Sturgis, formerly of Connecticut, now shares with the present writer a laboratory in the new Palmer Hall, and is very busy working on myxomycetes, making colored drawings of innumerable forms. As a result of his work the boundaries between several so-called species are becoming decidedly obscure.

Palmer Hall, the great new building of Colorado College, is the wonder and admiration of all who see it. From quarters which would have disgraced a high school, the scientific departments have moved into those which would do credit to any university. It is not possible to do everything at once, and it must be confessed that the equipment is not yet nearly up to the standard of the building. At the same time, there are very good facilities for teaching, and the museum contains a large amount of useful material. As regards the means for research, it seems to me that they are even now sufficient to keep any ambitious investigator from idleness. Of course the great opportunities are in the country itself, with the splendid mass of Pikes Peak close at hand, easily ascended by means of the cog railway. In the college, the large series of fossils—especially Cretaceous—collected by Professor Cragin invites study. Most of the material is from Kansas and Texas, but it would be invaluable for comparison to any one engaged in the study of the Colorado Cretaceous. There is also the herbarium of the late Edward Tatnall, of Wilmington, Delaware, which, although not rich in Rocky Mountain plants, is, on the whole, remarkably good, containing apparently most of the standard sets from the United States and Mexico which have been distributed in recent years.

The literature on biology at present possessed by the college is very insufficient, though the library contains many good things. There

is, however, Professor Sturgis's botanical library, very complete for the fungi, and including, I think, all the standard exsiccati, even those of Europe. The books include a complete set of Saccardo, which is now so difficult to obtain. My own library is nearly complete in those groups (Coccidæ, wild bees) which I have especially studied, and contains much besides, among other things the *Zoological Record* from 1889 to date.

On the whole, therefore, Colorado Springs offers good opportunities for resident work along several lines; and I presume the facilities will be improved every year. The other Colorado institutions I do not know so well, but I have within the last few months visited the State University, the Agricultural College and the Normal School.

At the State University, at Boulder, I found Professor C. Juday in charge of the biology, the regular incumbent, Professor Ramaley, having departed on a tour round the world. I do not know very much about Dr. Ramaley's work, except that he has published some interesting studies of the epidermal tissues of flowering plants—a subject of particular interest in the arid west.* Professor Juday is doing some work for the Bureau of Fisheries, on the fishes of Colorado and their food, and the constituents of the plankton of the Colorado lakes. This work, of course, covers a field little explored in our state, and it is very fortunate that it can be undertaken by a resident investigator, though, as I understand it, his residence among us is only temporary. The university museum and herbarium are sufficiently good to be very valuable for teaching purposes, but from the standpoint of an investigator they are disappointing. Perhaps the most pleasing thing in the collection is a nice series of local birds, with full explanatory labels. Judge Junius Henderson, the curator of the museum, has devoted a good deal of attention to the birds, and also to paleontology. The new library building of the university is extremely beautiful and the library arrangement and facilities for getting at the books could scarcely be bettered. I noticed among the books a set of the *Challenger* Reports, *Nature* from the beginning, all of *Pittonia,* Edward's 'Butterflies of North America,' and many other good things.

The Agricultural College, at Fort Collins, is chiefly noted biologically for the entomological work of Professor Gillette and his former assistants Professor Ball, Mr. C. F. Baker and Mr. E. S. G. Titus. From this institution have come the important 'List of the Hemiptera of Colorado,' Professor Gillette's revision of the Typhlocybidæ and many other works known to all entomologists. There is just now ready for publication the first part of a catalogue of the

* Since this was written Dr. Ramaley has returned from his journey round the world, bringing a large and most interesting collection from Java, Ceylon, Japan, etc. He is engaged in special researches on the anatomy of the cotyledon.

Orthoptera of Colorado. As might be supposed, the entomological collections and library are very good, although the latter does not contain everything I expected to see. Professor Gillette is at present assisted by Mr. S. Arthur Johnson, a relative of the well-known curator of the Boston Society of Natural History. Mr. Johnson is doing very nice work on the Hymenoptera, especially on their nesting habits and parasites. He has discovered, for example, the hitherto unknown nest of *Entechnia,* and has definitely proved the association of *Triepeolus* with *Melissodes.* Also with Professor Gillette is Mr. Chas. Jones, a young entomologist who will be heard of in the future. Last summer he worked in a mine at Silverton, Colorado, and spent his leisure moments making by far the largest and best collection yet made of the insects of the Arctic-Alpine zone in the Rocky Mountains. In the Department of Botany and Horticulture at the Agricultural College, Professor Paddock is properly a horticulturist; but his assistant, Mr. F. M. Rolfs, a brother of Professor Rolfs, of Florida, is doing some very interesting work on parasitic fungi. The herbarium of the college gave me much surprise and pleasure. The last time I saw it, several years ago, it was in such a condition as to be of little use for critical work. Now, the Colorado material in it has all been gone over by Dr. P. A. Rydberg, of New York, who has in press a list of the flora of Colorado, *i. e.,* of the flowering plants thereof. The greater part of the named material has been returned to the college, and I was naturally very much interested in the determinations. Although, as I learned from Professor Paddock, the college herbarium contains only about half as many Colorado plants as they have in the New York Botanical Garden, it is by far the best and most useful public herbarium in the state. I say public herbarium because Mr. Geo. Osterhout, of New Windsor, Colo., has long studied the native flora, and is said to have a very fine collection. He has described quite a number of new Colorado plants. At the Normal School, at Greeley, they do not pretend to do much research, but Professor Beardsley has made some studies of the minute fresh-water Crustacea, and of the Protozoa, describing some new species. He has also made a collection of Colorado reptiles and amphibia, and will, I believe, publish a list of them. The library of the Normal School is very well arranged, and contains some good zoological books I did not expect to see.

In Denver, the State Historical and Natural History Society has a collection, poorly housed in the lowest floor of the capitol building. Mr. Ellsworth Bethel, of the Denver West Side High School, has long studied the fungi and flowering plants of Colorado, and has a large collection. He has discovered very many new species, especially among the fungi, but his duties leave him little time for research. The East Side High School in Denver has a herbarium, presented by Miss Alice

Eastwood, the well-known botanist of California, who used to teach in Denver, spending her summers studying the Colorado flora.

In New Mexico, biology is not very much studied. I will only refer at this time to Professor E. O. Wooton and his assistant, Mr. Metcalfe, at the Agricultural College. These botanists have made large collections of the New Mexico flora, and Professor Wooton's writings on the subject are well known.

In Wyoming, one thinks first and last of Professor Aven Nelson, the indefatigable botanist of the University of Wyoming. The herbarium he has accumulated there is by far the best within our region, and his critical studies of the Rocky Mountain flora in the field have given him a knowledge possessed by no other man. He has, of course, described very many new species, and I have heard it stated that he will cooperate with Professor Coulter in the production of a new edition of the latter's 'Rocky Mountain Botany,' now so greatly behind the times. Between Professor Nelson and Dr. Rydberg we seem likely to possess in the near future works which will give a new impetus to the study of Rocky Mountain plants, making easy that which has been getting increasingly difficult. Professor Nelson has already issued a small school flora, including only the commoner and more conspicuous plants.

In Montana, we have Professor Cooley, the entomologist of the experiment station, and Professor Morton J. Elrod, of the University of Montana. The work of Professor Elrod in founding a biological station and studying the mollusca, dragon-flies, etc., is extremely valuable, and one may hope that it will continue to find hearty support. The publications of the University of Montana show that Professor Elrod has been able to interest a number of persons in the 'biological survey' idea, and the work seems to be growing in volume and value every year.

I have not attempted to refer to every Rocky Mountain worker, nor have I said anything about visiting naturalists; but it would be a serious omission not to allude to Dr. Clements, of the University of Nebraska, who for a number of years has been a 'summer resident,' as they say of certain birds. Dr. Clements migrates to the mountains when his teaching work closes in Nebraska, and, with others, occupies a cottage at Minnehaha, which is on Pike's Peak, at an altitude of 8,400 feet. From this point he explores the slopes of the peak and the surrounding country, and makes ecological observations.

The above brief account of Rocky Mountain biology will make it apparent, I hope, that there are at least six places where fairly good facilities, of one sort or another, are offered for biological research. These are the University of Montana, the University of Wyoming, the University of Colorado, Colorado College, the Agricultural College of Colorado and the Agricultural College of New Mexico. In some in-

stances, *e. g.*, the botany at the University of Wyoming, these facilities are extremely good. It will also be clear that there are several resident naturalists within our area pushing forward the work to the best of their ability. Thus the outlook is in many ways satisfactory, but there are still great difficulties to be overcome. It is evident that the men already in the field can not nearly cover it; instead of a dozen or so, we need at least a hundred active workers, and a thousand would not find their hands idle. This is utopian talk, of course; but I do think that the first need is to increase the working force. Then again, those who are at work, almost without exception, have to get their living in other ways, and thus can give comparatively little time to research. In the experiment stations, research is well provided for, but the popular clamor for 'practical' investigations and immediate results usually prevents the undertaking of anything very broad or fundamental. Furthermore, the experiment station officers mostly have to do a large amount of teaching. In the colleges and universities, teaching is naturally to the front, and in our mountain states this does not mean the teaching of graduate students to more than a very limited extent. A short time ago I appealed to the professor of chemistry in one of the Colorado institutions to do a piece of work of scientific and economic value. He immediately said that he longed to do it, 'but what can I do? I am giving seven courses!' This is a fairly typical case, and although I know very well there are many who would not do anything as investigators if they had the time, the fact remains that those who would, and in fact do, are handicapped to an extent little appreciated or understood. It is not that research is disliked; if anything is done it usually meets with approval, but it is not understood that it is fundamentally necessary to progress, and that it requires time as well as space to flourish in. Much of what has stood for culture in the west has been little better than a sort of intellectual parasitism on the east and Europe, and there is not yet an understanding or appreciation of the efforts to form an endemic product.

On the other hand, those who have accumulated wealth, or in some manner have acquired the means of living at the expense of others, will find in the mountain states ample opportunity. There are, of course, many such people, but with very rare exceptions they do not take to biological subjects. The well-to-do amateur is, for some reason, extremely scarce among us, though in England, for instance, his kind has done wonders. Thus there is plenty to praise and plenty to blame; but the only thing to do is to go ahead, and if the car of progress moves slowly, at all events it perceptibly moves.

WHAT IS RESEARCH?

By Professor HENRY SHALER WILLIAMS,
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THE faculty for research is not some peculiar power of the mind, possessed by some and not possessed by others, but it is a common faculty of all intelligent minds, more active often in youth than later in life, and its exercise in forwarding research depends more upon its discipline and training than upon any so-called original endowment of its possessor. But research is diverse from study, and the legitimate outcome of its exercise is not learning what is already known, but the extending of the boundaries of knowledge beyond the point reached by others.

In order that these faculties may be appreciated and, when in a promising state of development, may be properly trained, it is requisite that the faculties be clearly recognized and understood, and that the kind of exercise necessary to strengthen and develop them be known and appreciated.

What, then, is the particular part of our mental processes by which research is accomplished? An answer to this question may be reached directly by distinguishing research from its most closely related activities, namely, investigation and study.

Study has for its direct aim the acquirement of knowledge; investigation has for its aim the understanding of the reasons and relations of things already known; full comprehension and scientific discernment are the results of its exercise. Pure research beyond both of these has for its aim the discovery of facts, truths and relations not previously known; its results are the extension of the field of knowledge.

Study, when separated from investigation and research, primarily deals with language and the names of things and ideas. Close attention to words and language and a careful cultivation of the power of memory are essential to one who would become a perfect student. In this statement 'words and language' are not restricted to what are technically called the languages, as French, German, Greek, Latin or English, but it applies to the sciences as well. The study of chemistry, botany. or astronomy primarily consists in learning the proper words and names to apply to definite objects, or phenomena of nature. The task of the student of science is the learning of the nomenclature and formulas of science, and such learning may be acquired without much

comprehension of the laws of nature or understanding of the principles which are symbolized by the language which the student may have acquired ability to use fluently. In other words, a full knowledge of the names of things may be acquired by study, together with ability to talk fluently about them without much personal comprehension, or, as we say, grasp, of the things described. This possibility I have seen realized particularly among students who from their youth have been rigidly trained in the old classical method of education. Men trained strictly by this method understand words with precision and acquire a full vocabulary. They may be able to speak eloquently on subjects of which they know little. I have seen such scholars (juniors or seniors in college), who upon receiving the clue to the subject on which they were expected to write examination papers, could spin out a creditable set of answers quite superior to those written by their less trained (literarily) companions, whose real comprehension of the subject, as shown by other tests, was vastly superior to their own.

The pure student also may be said to be hindered from doing his best by stopping to investigate either the meaning or the application of the simple statements recorded in the verbal text before him. This learning by rote is an evil result of carrying study to excess, but it is also a definite aim of study pure and simple. A second result, more or less evil and which comes also as a natural result of pure study, is the tendency to lead the learner to accept, without question, the correctness of the statements he learns. He learns to depend upon others for his knowledge, and thus the expression 'getting knowledge at second-hand' becomes an actuality for the pure student. Although a successful student, he may fail not only to comprehend what he learns, but fail to think for himself. Nevertheless, the learning of other people's knowledge is an essential step in educating one's intellectual powers for higher work. We must have a wide knowledge of words and their meanings, and a good facility in the use of language, before we can successfully carry on systematic thought; and the more full and precise his vocabulary the wider and deeper will be the possibilities of both the investigator and the man of original research. Study is thus an essential preparation for investigation; but if the two be mixed too early in one's educational progress, the results will be deficient by virtue both of the inaccuracy of the learning and of the immaturity of the investigation.

A student who begins to investigate too early will, on the one hand, degrade the quality of his scholarship, and, on the other, he will find that the immature conceptions of science which he first forms must often be abandoned upon fuller investigation; or if retained will lessen the value of his results and place him in secondary rank as an investigator.

To attain the best results of study one must become a docile pupil, learning with precision and thoroughness exactly that which the words and language present to him. Taking statements exactly as they are formulated and using words exactly as others have used them before us—only so may be laid the solid foundation on which genuine research can be securely built.

Investigation Distinct from Study.—Investigation is distinctly another mental process from pure study. Its results are different from learning, as different as digestion is from eating, and for its best exercise a difference should be made in the training process, and in the objects to which the attention is given.

Learning has to do with words and language. Investigation deals with meaning, ideas and conceptions. One of the first acts of the investigator, after he has learned to know the thing or phenomenon by its description, consists in transforming the description into new terms. It is like taking a crystal and turning it into a new light in one's hand to see the new reflections due to changed position.

The attempt to express the conception in different language leads to a fuller realization of that which is contained in the description, as distinguished from the description itself. This result is more easily gained when an actual physical object before the investigator is impressing his senses, than when words alone are used. In the field of philosophy and in mathematics, the process of investigation may be carried on without a physical object being present, except in imagination. In this case the discipline of the mental faculties is more direct, and for discipline alone it may possibly be better than the laboratory method. The rudiments of investigation are found in the classical mode of education, as in translating Greek or Latin into English. But investigation methods may be exercised and trained, certainly more easily and with greater pleasure to young minds, when the visible object is before one, as in the laboratory, where it remains constant and can be tested over and over again, while the terms of expressing one's own view of it are gradually perfected. The laboratory is the special place of investigation.

Pure investigation deals with knowledge already attained; as with study the acquisition of the investigator is an acquirement of facts of common knowledge and is not yet research. Experiments are made not to advance knowledge, only to advance the knowledge of the experimenter in fields already familiar to the teacher. In the fields of science, with which I am particularly concerned in this discussion, there is this difference between study and investigation, that investigation deals with the objective things as distinguished from the words describing them. Phenomena are but phases of things, and are included within the general term things, as being together experienced by our senses, inde-

pendently of any names or language by which they may be symbolized. On the other hand, it is important to notice that things and their phenomena are grasped by the mind in the same way that words and language of common speech are learned. By study we gain knowledge of words, by investigation knowledge of the things and phenomena of our experience. The laboratory, the museum, the world at large are the normal fields of this process of investigation; much as books and the words of the lecturer are the normal fields of pure study. This brings us to the definition of research.

Original research goes beyond investigation in that the things sought for are not only undescribed, but, when the research begins, are actually out of sight, that is, unseen. The field of research may be visible, but the genuine meaning of research is a looking beyond what can be seen; herein is found the most essential characteristic of successful research, viz., a comprehensive, a keen and a disciplined imagination of things and truths before they become objects of experience. We must distinguish, too, between scientific research and haphazard stumbling upon strange things in out of the way places. Curiosity hunters may discover novel and undescribed things, and may even point the way to their source, but it is the trained scientific investigator who discriminates their true value, orients them in the known world of things and makes them available for the use of man.

Research, as was noted at the outset, is not a special faculty possessed by a few, but a common faculty specially trained and systematically exercised by but the few, for whom it becomes a tool of the highest value, and the means of opening up new fields of knowledge to mankind.

The underlying principle of original research is simple inquisitiveness; that trait so characteristic of the Yankee and the fox. I use the term Yankee as the name for a typical American, not a local or political term, but the name for the smart, shrewd, inventive man, who depends upon his own resources and, if without learning or education, still succeeds in penetrating untried fields, and in making headway under all manner of reverses, hindrances and difficulties, always exhibiting a quickness to observe differences and to interpret the meaning of things. All kinds of successful pioneers are made of such stuff.

This quality is generally more active in youth than in grown men; the common methods of education repress rather than encourage its activity; and the old classical system of education is particularly effective in this direction. This repressing result is reached, however, not by direct means, but by the very perfection with which study, pure and simple, is fostered. It is a conspicuous fact in schools that often the keenest and brightest boy is not always the best student; he may know more and observe more closely than any other, but he gets low marks in spelling, reading and, may be, in arithmetic, even in geography as

it was formerly taught, and particularly in grammar, which, I believe, is not now made so much of as formerly in common schools. The education which represses curiosity and inquisitiveness, and only educates memory for words, makes learned men of the pedantic kind; so far it discourages and neglects the discipline of the research quality. The fact that the classical system of education trains the study quality to the neglect of research does not, however, diminish the importance of the kind of training it supplies as a fitting for a man for research. It is the neglected part to which attention is directed. I know of no better kind of discipline in study than the thorough and refined methods of the old classical system, supplemented by an early and continuous use of science. It is the neglect of this method which makes the slipshod, careless work which all scientific scholars regret, and unfits many students of science for successful investigation or research. This discipline produces results which may be likened to the tempering of steel, which shows after the steel is hammered into shape and sharpened for its specific purposes. The man who lacks this tempering is incapable of holding the keen edge, or of making the fine and far-reaching discriminations, which a mind well tempered by the rigid discipline of the classical system has acquired.

From the objective side research is the attacking of unsolved problems, the examination of facts undescribed and unexplored, the seeking for truths imagined, but not hitherto formulated. New discoveries of truths, the correction of partial statements of truth, the formation and formulations of new conceptions, these are the results of research. What are the disciplines which foster research?

The first requisite in the discipline required for successful research is the keeping alive of the original faculty of inquisitiveness. It must not be stifled while the student is being taught language and the content of language from the books.

The second point is that the method of exact study must be thoroughly acquired, and applied in a wide field of knowledge, whatever may be the particular field of original research later to be chosen. One of the greatest difficulties met with in selecting men to take up original research in particular fields (as brought to light in the deliberations of the Carnegie Institution) is to find men sufficiently well trained to be competent to go on without guidance in new and untried fields. It is also a great mistake, since it necessarily leads to later disappointment, to tempt, or allow, unripe men to try their hand at deep problems of research—to putter over serious problems which the expert and experienced hand knowingly hesitates to attempt. There is no better way to acquire this part of the discipline than by a thorough classical training, such as might be given in what is called an arts course in our college, with a carefully selected and systematically

arranged series of science studies added. In this study of science care should be taken to begin courses in which lessons are to be learned, and learned upon authority and with exactitude, later to be perfected by laboratory practise; such is, in my judgment, the finest kind of preliminary discipline for a man of research. For it takes a learned man to tell the truth with precision, even when he is its discoverer; and, moreover, only the learned man knows when he has discovered a new truth. Ability to study deeply and accurately is then the second essential qualification to the making of a man of research.

Thirdly, to undertake original research a man must be a trained investigator. He must know the methods by which other men have discovered truths and interpreted things. To learn this he must have gone through the exercise of a personal discovery of the meaning of things —have tested for himself the reality of the descriptions written in the books. Such training is best given in the laboratory; analysis and experiment leading to already known results must be gone over by the investigator in careful detail, and the steps of the progress, the associated conditions, the order of sequence of phenomena must be closely observed and recorded; and the relation of the phenomena to one another and the results of experiments clearly understood, formulated and, best of all, fully written out.

Fourth, the man of research should have a vivid imagination, which should be trained to be accurate and to be his servant, not his master. This faculty, I fear, is often trained out of men by what is called experience. Not only does the dry, matter of fact, world of every day tend to keep one down to thoughts of the immediate present, but the immensity of science and its practical applications, by the very abundance of the known facts, crowds out of use all mental pictures of hypothetical conditions not known to common experience. Nevertheless, as has been already noted, the very function of research is to go beyond the field of present knowledge, and in it the attention must be fixed steadily upon concepts, the realization of which has not yet been attained. The scientific imagination may be exercised and disciplined by the study of mathematics. The architect's work is a definite application of imagination to projecting new construction. What are called 'working hypotheses' are the results of this exercise of imagination in advancing research. The discipline of the faculty of imagination is necessary to enable the researcher to distinguish between his concepts of imagination and his concepts of experience. If he knows how to distinguish them, his imagination becomes his valuable assistant, if ignorant or unobservant of the difference, his results become speculative and ineffective.

A fifth trait marking the typical man of research is a wide, open mind. Philosophically he should be a whole man, not simply a one-

sided specialist. Many a man attempting research has come short of really advancing knowledge on account of his prejudices. Darwin was a typical example of a philosophically whole man; whatever personal opinions he may have had, they were never allowed to prejudice any hypothesis he was examining, nor to interfere with conclusions toward which the observed facts logically led him.

Faraday was another such man. He wrote the following words early in his scientific career and his life work was an expression of their truth:

> The philosopher should be a man willing to listen to every suggestion, but determined to judge for himself. He should not be governed by appearances, have no favorite hypothesis, be of no school, and in doctrine have no master. He should not be a respecter of persons, but of things. Truth should be his primary object. If to these qualities he added industry, he may indeed hope to walk within the veil of the temple of nature.

Again he wrote:

> We may be sure of facts, but our interpretation of facts we should doubt. He is the wisest philosopher who holds his theory with some doubt; who is able to proportion his judgment and confidence to the value of the evidence set before him, taking a fact for a fact and a supposition for a supposition. (Gladstone's, 'Life of Mr. Faraday,' pp. 93 and 94.)

Sixth, research must be protected from interference. I take this expression from the game of football; the man who runs with the ball requires the protection of all the rest of the team. So research can not be carried on to successful issue, except by a man who is permitted to devote the best that is in him to the problem of research, and to do so he must have the way opened for him. Many may help to give him the opportunity, but the one thing essential is that he be left free and unimpeded to pursue the problem, wherever it leads him. Time and occasion and money must be at his disposal to such extent as his problem demands.

Research work must be regarded as the very flower of a university system, and should not be lightly valued or carelessly managed. Only men thoroughly fitted by the training of study and the training of investigation, and the training of mental discipline, should be allowed the privilege of entering upon research in our universities; but whenever the proper man is found, the providing of a way for his pursuit of research work, in the field for which he is best fitted, becomes a contribution direct to the progress of science in the world, of which any university may be proud. The university may well scrutinize with consummate care the qualities demanded for research, provide rigidly for the discrimination of those qualities wherever they are highly developed, and may wisely provide with liberality for the true man of research when he is discovered and is properly trained for his work. But in

order to help the right man in his work of research, too great care can not be given to the importance of study and investigation as preliminary steps in the preparation for research. A brilliant student, who lacks power of investigation, may be unfit and inadequate to carry on research; and the most capable man of research may still come short of being a ready or able learner of words. Many a man by indomitable energy may overcome great deficiencies, but this fact should not excuse a university from the most rigid discrimination of the essential elements of merit in a man seeking to undertake research work.

As the highest aim of the literary scholar is the production of lasting literary creations, so the highest aim of the research scholar is the advancement of the boundaries of actual scientific knowledge and the discovery of new truths. In both cases the aim must be toward the highest, or the attainment will be unworthy. It is given to but very few to make marked success in either line. The man of research must be willing to devote his life to his work, to sacrifice most of the enjoyable things otherwise within his reach. He must not be deceived as to the measurement of success. Research can not be weighed by its practical value, to apply the term in its every-day sense, for practical value depends upon the financial productiveness of the energy expended. As with a newly discovered country, years of toil and great expenditure of money, and it may be loss of life, may be demanded before any profit results from the discovery. Even in fields in which rich results have already been attained great expenditure of thought, energy and expense may be required before practical results become evident from new research. We can recall many such cases. Success in research can not be measured by applause, or even by recognition from other scientific investigators. For appreciation comes only from those who appreciate; only those thoroughly conversant with a particular field of knowledge can distinguish an advance or enlargement of the boundaries of that field. The man of research must, therefore, be content to be alone in most of his work; unappreciated and unapplauded, using energy and money on tasks which may seem to all about him useless and wasteful. For these reasons this field of activity should not be entered upon lightly.

We as teachers should ever be on the watch for men of the right quality for such advanced work, but should never tempt mere enthusiasts to undertake a task which for success requires the toughness of a soldier, the temper of a saint and the training of a scholar.

PLANTS THAT HIDE FROM ANIMALS.

By Professor W. J. BEAL,
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Plants Protected by Growing under Thorn Bushes and Thistles.—C. G. Pringle, for many years a famous plant collector, especially in Mexico and the arid regions of the United States, speaks of a native grass of northern Mexico, *Muhlenbergia Texana,* as such a great favorite with all grazing animals that it is usually exterminated or nearly so, except when growing under the protection of thorny shrubs, usually mesquite bushes. In Arizona during the winter and spring, the Indians bring it long distances into the towns to sell. He adds, 'How many times I have contended with the horrid mesquite bushes to gather an armful of this grass to carry joyfully to my hungry and jaded horses.' In such cases the thorns, spines and perhaps bitter taste of the bushes not only protect the young growth and leaves of certain plants, but furnish shelter for other tender and nutritious herbage.

In arid regions, especially, similar instances of protection by thorn bushes are numerous.

Plants retire beneath the Surface of the Ground and are protected from Animals.—At the close of the growing season, large numbers of plants, especially in regions subject to protracted droughts or to severe cold, cast off their leaves, take on a condensed form and remain secure beneath the surface of the ground for months at a time in the form of bulbs, tubers and rootstocks. At such times they are nearly sure to escape destruction by animals. I only need to mention as examples Solomon's seal, Dutchman's breeches, May apple, goldenrod, artichoke.

Plants Protected from Animals by Water.—Not only the flowers of many species of plants as they project above the surface of the water are protected from most unwelcome insects, but the whole plants as well.

Mud turtles, certain fishes, water snails, larvæ of insects, eat aquatic plants, but most other animals are unable to reach them in such places.

Water-plantain, wild rice, pond lilies, arrow-head, pickerel-weed, pondweed, lizard's tail, bulrush, bur-reed, cat-tail flag, water dock, and many more of their associates, root at the bottom with leaves floating on the surface or projecting above.

Innumerable low forms, known as algae, are at home in lakes, ponds and streams or on the surface of the water, while other kinds thrive in salt or brackish water. These aquatics find protection below

the surface or by extending above it, not only from numerous animals, but they have no competition with others which can only grow on dry or moist soil.

Aquatics and marine plants and algae are also protected from extremes of cold and heat. During the winter of a cold or temperate climate the rootstocks and buds severed from the tips or branches, and even the entire plant of some species, remain safe and dormant in the mud at the bottom, ready on the approach of mild weather to begin growing again.

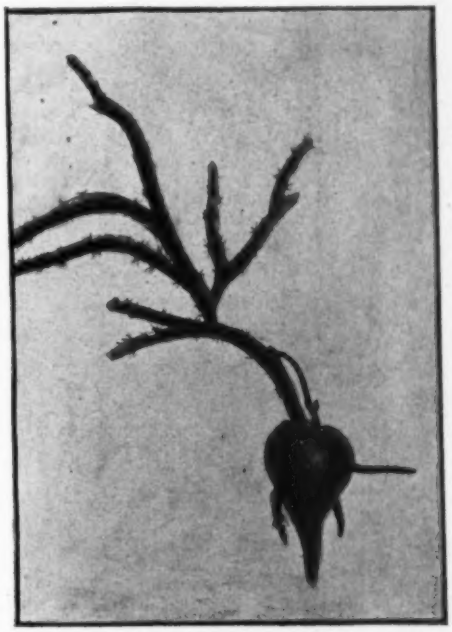


FIG. 1. A NIGHT BLOOMING CEREUS, *Cereus Greggii*, SHOWING THE LARGE SUCCULENT ROOTS WHICH SERVE AS A STOREHOUSE FOR WATER (Much reduced.)

Some are amphibious, able to thrive when the land is flooded or when the floods have subsided. Plants with such habits have little competition.

By climbing Trees and Bushes Many Vines get beyond the Reach of Cattle.—A considerable number of plants practise economy by growing slender stems instead of producing large tree-trunks for self-support.

Great numbers of climbing plants or vines are favored above some others in their ability to reach the light and thrive, even though their stems are very weak and slender. This habit brings most of the leaves and flowers of many of them beyond the reach of cattle and other herbivores.

Plant Traps in the Water catch Fish and Worms to eat.—A few plants not only defend themselves, but are aggressive fighters because they put to good use the animals they capture.

The bladderwort is a water plant and catches much of its food. Underneath the surface of the water in which the plant floats are a number of lax, leafy branches spread out in all directions and attached to these are large numbers of little flattened sacks or bladders, sometimes one sixth of an inch long. The small end of each little bladder is surrounded by a cluster of bristles forming a sort of hollow funnel leading into the mouth below, and this is covered inside by a perfect little trap door, which fits closely, but opens with the least

pressure from without. A little worm or insect, or even a very small fish, can pass within, but never back again. The sack acts like an eel trap or a catch-'em-alive mouse trap. These little sacks actually allure very small animals by displaying glandular hairs about the entrance. The small animals are imprisoned and soon perish and decay to nourish the wicked plant.

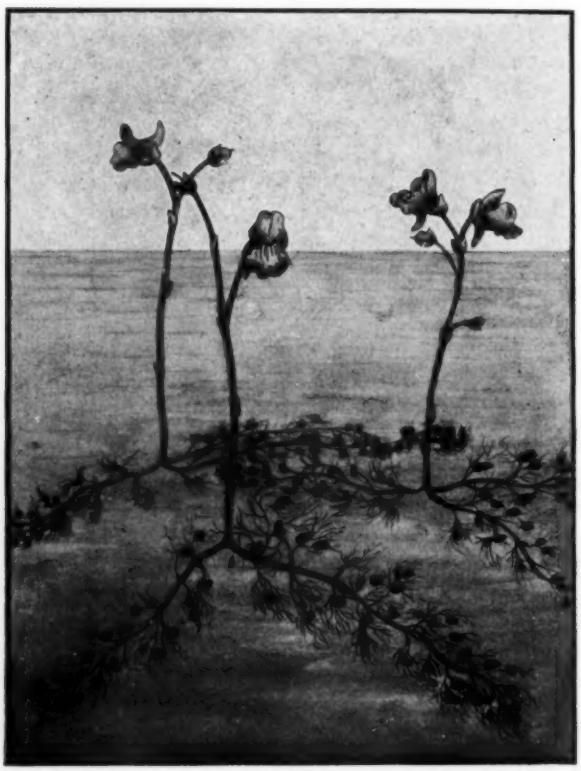


FIG. 2. BLADDERWORT, *Utricularia vulgaris*, A FLOATING AQUATIC, THE FLOWERS, AT LEAST ARE PROTECTED FROM CRAWLING INSECTS. (Slightly reduced.)

It is not the purpose of these pages to go into details regarding the methods of insectivorous plants, but to tell how plants defend or protect themselves.

When scattered by Bursting Pods, the Seeds are seldom found by Animals.—When mature, the pistils of certain plants burst quickly and with considerable force, scattering the seeds in every direction. The small size and the inconspicuous colors of the seeds make it certain that few of them will ever be found and destroyed by insects or mice. Plants of this kind are euphorbias or spurges, violets, peas, beans, witch hazel, castor-oil plants, balsams and many more.

The bearded chaff enclosing the grains of porcupine grass and wild

barley are irritating to the mouths of grazing animals, which attempt to eat them, though it is not probable that cattle avoid these plants on this account.

Gardeners often overlook some of the Weeds.—For many years the writer has had the oversight of two or three acres on which were grown some 2,000 kinds of plants. It is the exception to pass over a bed after a workman has 'dressed it up' and not find a number of weeds left among the cultivated plants. They are overlooked because of some resemblance of the weed to the plant desired. I enumerate a few examples found one day in the month of May: A few wild onions are left in the asparagus; wild seedling lilies in a plat of Solomon's seal and in a bed of turtle-head; June grass lurks in plats of several sorts of pinks, of *Phlox* and of many other plants; narrow-leaved dock is often abundant, and some of it is left in a plat of dandelions, of teasels, of rhubarb, of buttercups, of rue anemone; pig weeds are left to go to seed among potatoes and tomatoes; the brittle joints of prickly pear are left to grow among other species which they resemble; seeds of violets in variety spring up in plats of other violets where they were shot by the mother plants; chickweeds are rarely ever all discovered in plats of speedwells; while speedwell lurks among the snap-dragons; white clover is not all removed from plats of alsike clover, red clover and black medick; young plants of climbing fumitory are left in beds of ginseng, Dutchman's breeches and yellow puccoon; seedling wild cherries are overlooked among winter berries; ground-nut escapes notice as it comes up among hog pea-nuts, vetches, or

FIG. 3. LEAF AND FLOWER AND RIPENING FRUIT OF THE LOTUS, *Nelumbium*, PROTRUDING ABOVE THE WATER, WHICH PROTECTS THESE PLANTS FROM MANY ANIMALS. (Much reduced.)

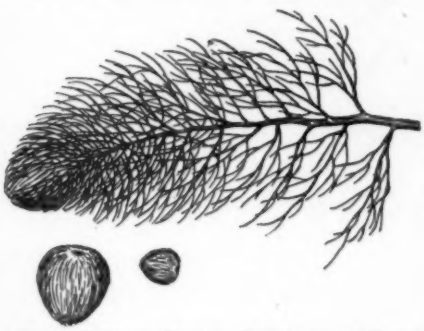


FIG. 4. BUDS OR TIPS OF BRANCHES OF BLADDERWORT SEPARATED FROM THE MAIN STEM AND ARE SOON READY TO SINK TO THE BOTTOM OF A POND AND THERE REMAIN DURING THE WINTER.

wild peas; matrimony vine is left to grow awhile among plants of bittersweet, ground cherry, horse-nettle and buffalo bur; the Oswego tea

FIG. 5. TWINING PLANT, WILD MORNING-GLORY. (Reduced.)

shies its rootstocks all about and many appear and remain for a time with peppermint, spearmint, sage, origanum and motherwort; wormwood visits ox-eye daisy and the two agree well together; Canadian golden-rod creeps into plats of low-land grasses, sedges, wild asters and artichokes; the enterprising dandelion is found in plats of hawkweed, chicory, wild lettuce sow-thistle, and is often overlooked; and squaw-weed finds a welcome with sweet-colt's-foot.

FIG. 6. A CROSS SECTION MUCH ENLARGED OF A BLADDER-LIKE TRAP WHICH CATCHES WORMS. (After Darwin.)

An Iris that imitates a Rattlesnake.—In portions of Washington rattlesnakes are very abundant, and are much dreaded by cattle and horses which graze large portions of the state.

In this region grow large quantites of *Iris Missouriensis* and when ripe the rattle of the seed in the pods closely resembles the rattle of the snake. Grazing animals invariably step back after hitting these pods, and thus the green leaves of the plants are spared to work for future crops of seeds.

For this observation I am indebted to Matt Crosby, of the U. S. Bureau of Forestry.

Seeds that mimic Pebbles.—Seeds are frequently met with that are mottled or striped or of an inconspicuous color difficult to find when dropped on the soil or among small pebbles. Seeds of this character are least liable to be destroyed. By a process of selection for many generations, no doubt, seeds have acquired their present colors, and some of them are still undergoing this process.

FIG. 7. THREE SEEDS OF CASTOR BEANS, BROWN AND MOTTLED, NOT EASILY FOUND WHEN THROWN ON THE GROUND. (Slightly enlarged.)

Certain sorts of cow peas resemble the red soil found in some regions. Caster-oil beans have been mentioned as examples of those which are mottled. Some of the cacti have an irregular shape and a dull gray color much resembling stones of the desert.

On the coast of some of the Philippine Islands, a coarse briar produces beans more or less approaching a sphere in shape.* They are about the size of the finger tips of a man and some of them, like peas crowded in the pod, have two flat surfaces. The color varies from moderately dark to light drab, some giving a faint greenish tinge, while the luster of many is exactly that of chert pebbles. Nearly all the specimens show a series of approximately parallel darker lines passing around, very suggestive of stratification. All are quite hard, cut only

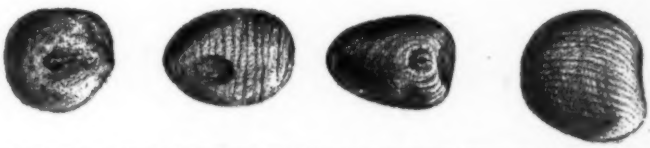


FIG. 8. REPRESENTING FOUR SEEDS WHICH CLOSELY RESEMBLE PEBBLES, AMONG WHICH THEY WERE ACCIDENTALLY GATHERED.

with difficulty with a knife, and when shaken together in the hand give that clinking sound, only somewhat duller, which is characteristic of pebbles. The mimicry then is that of mixed quartz pebbles, and covers *shape, size, luster, hardness and stratification.* It is so complete and perfect that it can not be regarded as mere coincidence. Placed in water, the beans are found to be buoyant. Undoubtedly this mimicry of pebbles has saved many a seed from destruction by fish, bird or reptile.

* W. H. Sherzer, *Bot. Gaz.*, Vol. XXI.

THE PROGRESS OF SCIENCE.

THE CARNEGIE FOUNDATION.

MR. ANDREW CARNEGIE has added to his vast gifts for public purposes a foundation to provide pensions for college teachers. He has selected twenty-five trustees, all but three of whom are heads of educational institutions, and has addressed to them a letter in which he states that he has transferred to them $10,000,000 five per cent. first mortgage bonds of the U. S. Steel Corporation to provide retiring pensions for the teachers of universities, colleges and technical schools in the United States, Canada and Newfoundland. Mr. Carnegie says: "I have reached the conclusion that the least rewarded of all the professions is that of the teacher in our higher educational institutions. New York City, generously, and very wisely, provides retiring pensions for teachers in her public schools and also for her policemen. Very few indeed of our colleges are able to do so. The consequences are grievous. Able men hesitate to adopt teaching as a career, and many old professors whose places should be occupied by younger men can not be retired." Strictly sectarian institutions and those supported by the state are excluded from participation.

This foundation opens up many problems of extreme importance. If it should be administered as a fund for indigent and disabled professors it would be an intolerable nuisance; but the trustees are of course too wise to permit any such outcome. Still it will be somewhat difficult to prevent it from becoming a charity. About ninety-five institutions are included in the preliminary list of those coming within the scope of the foundation. It would doubtless have been better to have distributed the money *pro rata* among such of these institutions as would agree to establish a pension system, and, as far as we can see, it would be best to distribute the income in this way. The obvious objection is that the demands of each institution would vary greatly from time to time. One of our leading universities with five hundred officers has a pension system, and we believe that there is at present only one professor on the retired list, whereas twenty years hence there may be a dozen. Still if the income were distributed among the institutions as a trust fund on condition that they establish a pension system, things would come out even in the long run. The expenses and machinery of administration would be reduced to a minimum, and the objectionable charity features would be avoided.

When an institution has a pension system, the professor who accepts a position in it does so under a business contract, and there is no question of any patronage or charity. Thus the statutes of Columbia University read: "Any professor who has been fifteen successive years or upwards in the service of the University, and who is also sixty-five years of age, or over, may at his own request signified to the president in writing, or upon motion of the trustees, be made an emeritus professor on half-pay from the beginning of the next succeeding fiscal year." When a man becomes a professor at Columbia University at the age of forty years, he has an expectation of life of about thirty years, and may look, say, to five years of half pay in

retirement. As part of his salary an annuity is paid for by the university at the rate perhaps of $300 a year, and his salary is that much larger than the sum he receives. The income of the Carnegie Foundation should be administered in some such way. One of the most important results of the scheme will be the pressure brought on the state universities to establish pension systems. The College of the City of New York has already provided liberal pensions, and the example will doubtless be followed elsewhere.

If eleemosynary features can be eliminated from the Carnegie Foundation, the matter is reduced to a phase of the world-wide conflict between individualism and socialism. Should the college teacher be taken care of by society, or should he take care of himself? Much can certainly be urged in favor of life tenure of office, fixed salaries and pensions for university professors. They are thereby set free to do their work, exempt to a considerable extent from anxiety over their material support, from commercial standards, from intrigues and possible injustice, from hasty work, from fear of the consequences of free speech. There are many who will develop the highest scholarship and produce the best research work under these conditions. But there are some who go to sleep comfortably in such a utopia and others who find it irksome. It tends towards dependence on the part of the professor and despotism on the part of the administration, to small salaries, to petty rivalries for honors in place of the serious competition of real life, to a kind of panmixia, where all are chosen who are called and there is but little selection of the best. Probably most people who take thought look forward to socialism as a necessary outcome of the increased complexity of social conditions, but there will be division of opinion as to whether steps in this direction such as Mr. Carnegie's foundation should be welcomed or regretted.

THE CONFERENCE OF ANATOMISTS AT THE WISTAR INSTITUTE.

THE conference of anatomists held on April 11 and 12 at the Wistar Institute of Anatomy, Philadelphia, portends an important step in the advancement of the science of anatomy in America. The men called to this conference differ widely in their interests, in their methods of work and interpretation, yet all are interested in the one great problem of anatomy in its broad sense. They were selected for this reason, as representing the various phases of activity in morphology. They were invited by the Wistar Institute of Anatomy at the suggestion of its director, Dr. M. J. Greenman, to meet in Philadelphia and discuss the relations which the institute might, with mutual advantage, bear to other forces in the promotion of anatomical research.

The following anatomists took part in the conference:

Dr. Lewellys F. Barker, professor of anatomy, University of Chicago, Chicago, Ills.

Dr. Edwin G. Conklin, professor of zoology, University of Pennsylvania, Philadelphia, Pa.

Dr. Henry H. Donaldson, professor of neurology, University of Chicago, Chicago, Ills.

Mr. Simon H. Gage, professor of embryology, Cornell University, Ithaca, N. Y.

Dr. G. Carl Huber, professor of embryology and histology, University of Michigan, Ann Arbor, Mich.

Dr. George S. Huntington, professor of anatomy, Columbia University, New York City.

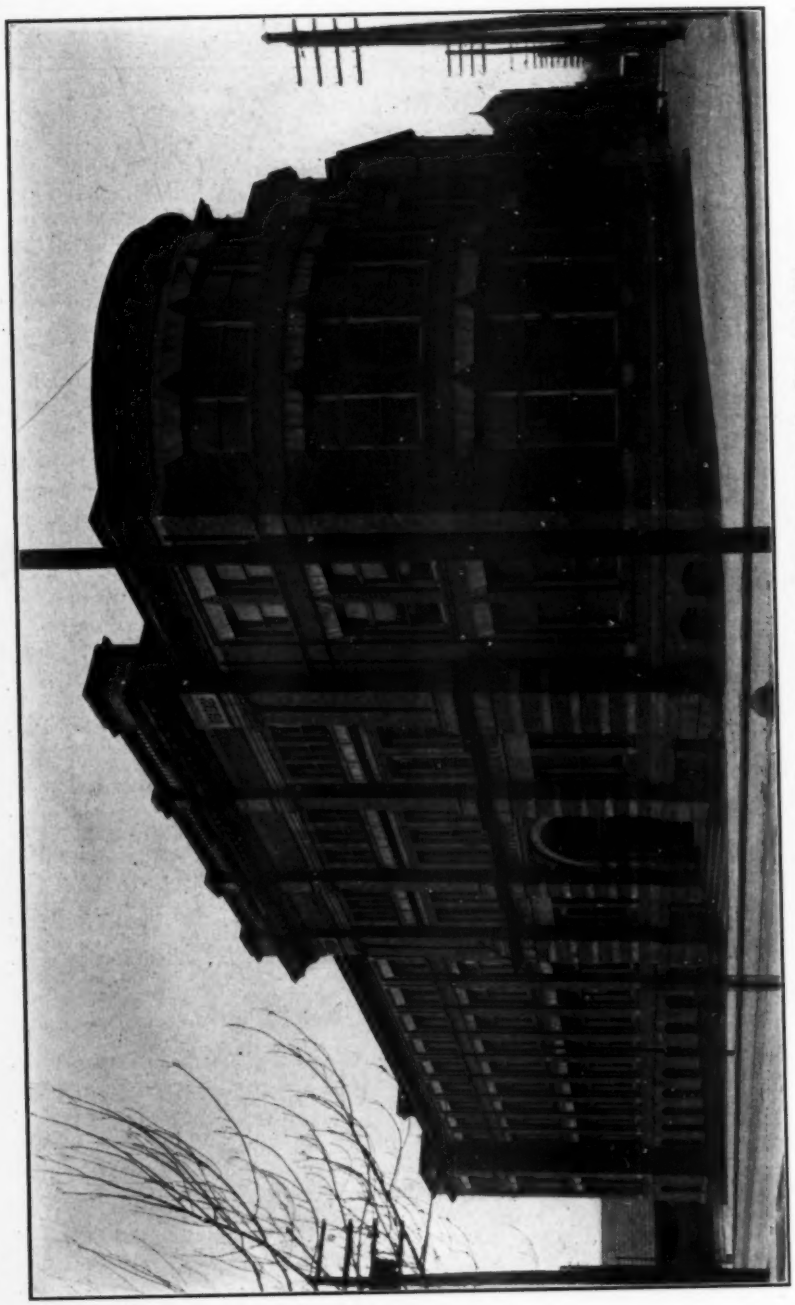
Dr. Franklin P. Mall, professor of anatomy, Johns Hopkins University, Baltimore, Md.

Dr. J. Playfair McMurrich, professor of anatomy, University of Michigan, Ann Arbor, Mich.

Dr. Charles S. Minot, professor of embryology, Harvard Medical School, Boston, Mass.

Dr. George S. Piersol, professor of anatomy, University of Pennsylvania, Philadelphia, Pa.

The belief of the institute authorities that there is much of common value to be gained by a cooperation of the institute with the anatomical forces of America is shared by many others, and it is the common opinion that the Wistar Institute, on account of its independent organization, will be of great value in supplementing the work

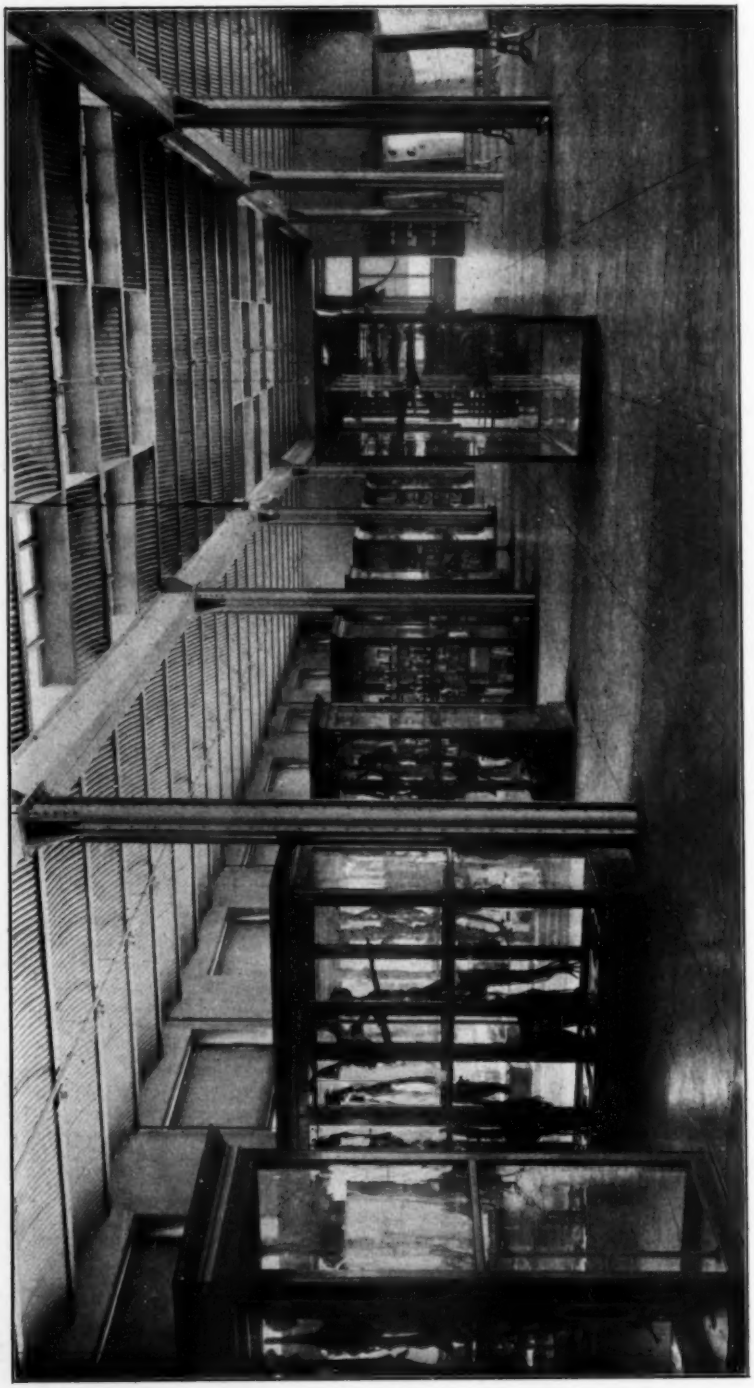


THE WISTAR INSTITUTE OF ANATOMY, WOODLAND AVENUE AND 36TH ST., PHILADELPHIA.

of other research schools and museums and especially in acting as a central institution for the collection and distribution of research materials and a storehouse for valuable morphological material which has been studied and is to be conserved for future comparisons.

The Wistar Museum was instituted in 1808 by Dr. Casper Wistar and incorporated as an independent institution in 1893 to foster and increase and make useful the museum originally known as the Wistar, or Wistar and Horner Museum—the first museum of human anatomy in America—and to promote researches in human and comparative anatomy. A charter was secured from the state of Pennsylvania, a modern fireproof building and endowment being given by General Isaac J. Wistar to maintain the equipment. This endowment has been most generously doubled and quadrupled several times over since 1893 by the same donor, almost without the knowledge of its board of managers. During the past twelve years the museum of the institute has multiplied its collections by six and their value by fifty, researches have been encouraged and much valuable work has been turned out, not to mention the great use which has been made of its museum (which is open free to the public), by students of medicine and natural history from the various educational institutions. The line in which the Wistar Institute has made decided progress is in museum technique; the development of new methods of exhibiting specimens, new dust proof steel and glass cases, new form of glass exhibition vessels, the value of which has been recognized both at home and abroad. Most of this work, especially the experimental part, is done in its own machine shops, and it is likely that a high degree of technical skill will be developed here. This in itself enables the institute to be of great assistance to research anatomy, especially in those

METHOD OF STORING VALUABLE OSTEOLOGICAL MATERIAL IN DUST-PROOF STEEL CASES; THE SPECIMENS ARE IN TRAYS.

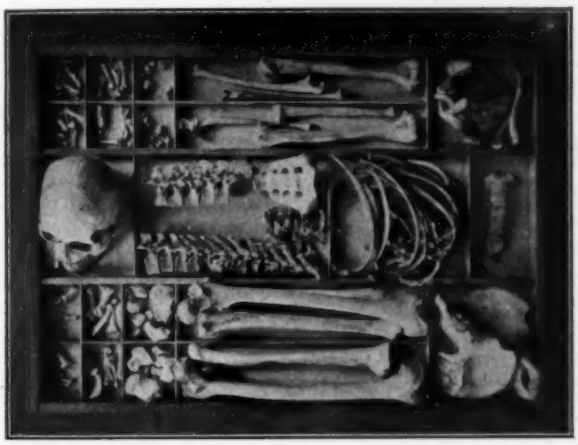


MUSEUM HALL OF FIRST FLOOR.

branches of the work where serial sections and other difficult preparations are to be made.

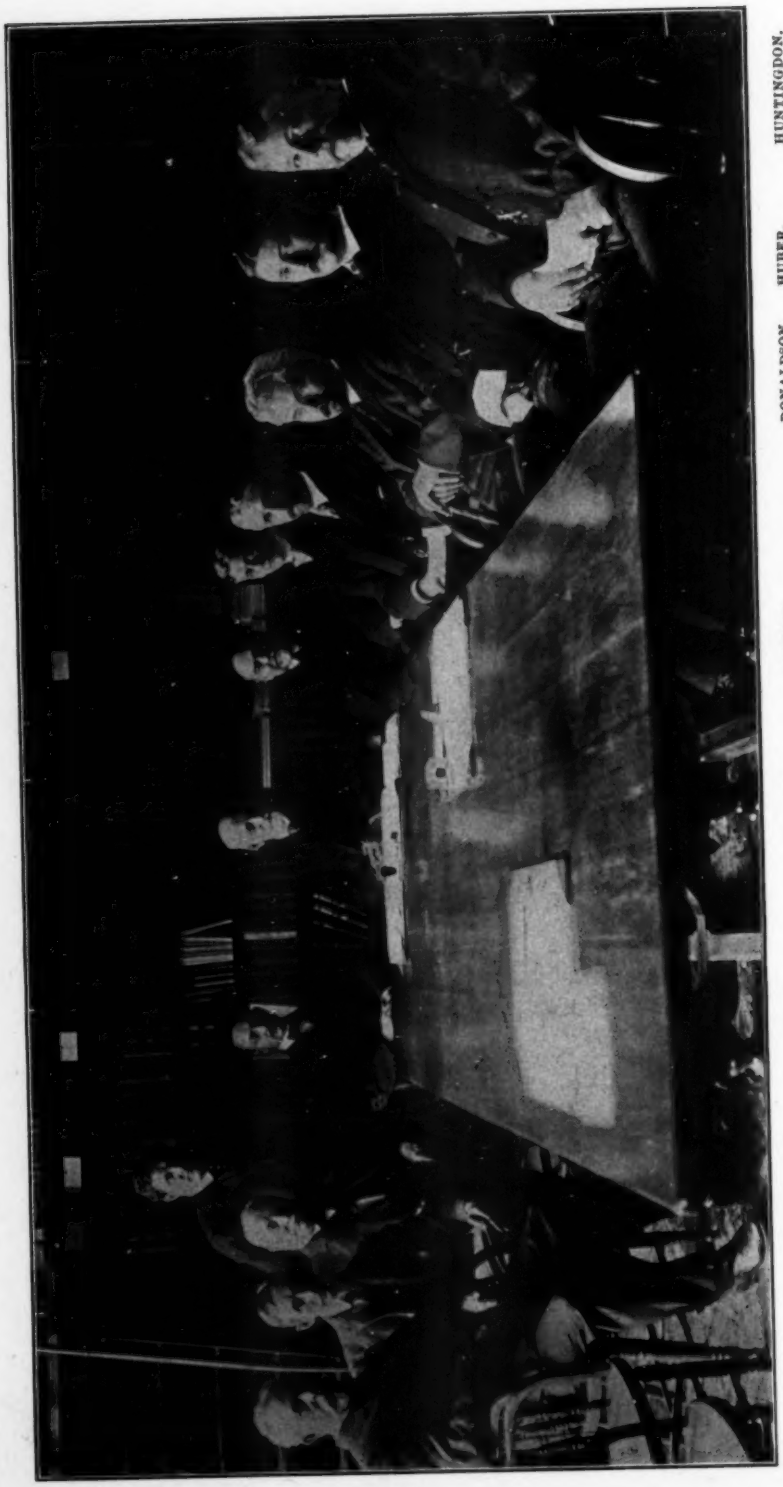
With an equipment for anatomical work equal to any in the country, with an endowment equal to the sum total expended by the three great anatomical schools in the states, with no energies expended in teaching undergraduate students, the Wistar Institute, organized as an independent research institution, stands unique in this country for the substantial support and encouragement of anatomy. How can it be made of greatest use to the science? This problem, coupled with the fact that there is no central institute for anatomy in America devoted solely to the one purpose, where research materials may be collected together, properly prepared and sent freely to interpreters who can not come to the institute, was the reason for calling together ten leaders in the science. They discussed it from their various standpoints and were unanimous in their opinion as to the work which might be accomplished. It was fully agreed at the first session that the development of a museum and the pursuit of research are inseparably united. A committee was then appointed to formulate certain suggestions to be discussed at the second session of the conference; this committee presented the following propositions:

Notes taken at the meeting of the committee yesterday are presented in the following order: (1) The principal object of the institute to be research and under these headings: (*a*) a chief of investigation, (*b*) research assistants or assistantships and men who shall divide their services between the museum proper and research, (*c*) technical assistants. (2) Research and materials: (*a*) research shall be in the field of neurology, (*b*) comparative anatomy and embryology. (3) Relations: (*a*) committee recommends that the subvention to the *Journal of Anatomy* be granted, (*b*) committee be appointed to consider relations of the Wistar Institute to American anatomists, (*c*) the Wistar Institute to apply to the Association of American Anatomists for cooperation. (4) That an advisory board of ten be appointed for general purposes: (*a*) to form a sub-committee on neurology as well as other sub-committees that may be needed, (*b*) to establish relations with the committee of the International Association of Academies for Brain Investigation and with other committees for collective investigation, (*c*) the committee recommends that the board bear in mind that while the general trend of work above outlined is recommended there is no intention to advise limitation of the functions of the institute to it exclusively.



METHOD OF STORING A HUMAN SKELETON IN A TRAY.

This report was briefly discussed on the following day and presented to the Wistar Institute as the finished work of the conference. The institute accepted it and suggested that the ad-



GAGE. MALL. WISTER. CONKLIN. MINOT. PIERSOL. DONALDSON. HUBER. HUNTINGDON.
GREENMAN (standing). BARKER. MCMURRICH.

CONFERENCE OF ANATOMISTS HELD AT THE WISTAR INSTITUTE ON APRIL 11 AND 12, 1905.

visory board organize at once and outline any work that might seem proper, pending the approval of the board of managers of the institute. This was done by electing Dr. Charles S. Minot as chairman and Dr. M. J. Greenman as permanent secretary.

The advisory board proceeded to appoint the following committees: on neurology and the establishment of relations with the International Association of Academies, Dr. L. F. Barker, Dr. H. H. Donaldson, Dr. F. P. Mall, Dr. J. P. McMurrich, Dr. C. S. Minot (this committee to elect its own chairman); on relations of the Wistar Institute to American Anatomists, Professor S. H. Gage, chairman, Dr. Geo. A. Piersol, Dr. G. Carl Huber; on comparative anatomy and embryology, Dr. Geo. S. Huntington, chairman, Dr. E. G. Conklin, Dr. F. P. Mall.

This move on the part of the Wistar Institute places its future development largely in the hands of a national board of leaders in anatomy, a feature as unusual as it is desirable, and through the Wistar Institute the work of anatomical schools may be supplemented and strengthened and brought into cooperation. If the plan is carried out as successfully as it has been started, there will be a decided increase in the efficiency of every effort put forth in the science.

It is expected that through the advisory board the facilities and opportunities offered by the Wistar Institute will be brought to the notice of active American anatomists, that difficult problems for cooperative research will be proposed, especially in neurology. Material will be collected, prepared and distributed to workers who are unable to come to the institute laboratories. The Wistar Institute asks nothing in return for its opportunities and the material it sends out, neither does it require papers to be published in any particular journal. The returns are sufficient so long as the science is aided, and the greater service it can be to research workers in the development and spread of original knowledge the more nearly will its purpose be achieved. That the Wistar Institute appreciates the cooperation and suggestions of the anatomists is shown by the prompt manner in which its board of managers created the advisory board and elected to its membership the anatomists who took part in the conference. It is understood that the suggestions made at this conference will be carried out in every detail as fast as the resources of the institute will permit.

SCIENTIFIC ITEMS.

We record with regret the deaths of Professor Otto Struve, director of the Poulkowa Observatory from 1862 to 1890; of Dr. Joseph Everett Dutton, who died in the Congo, where he was sent by the Liverpool School of Tropical Medicine to investigate trypanosomiasis and tick fever; of M. Henri de Saussure, the French zoologist; of Mr. H. B. Medlicott, F.R.S., director of the Geological Survey of India from 1876 to 1887, and of Colonel Nicholas Pike, known for his contributions to the natural history of birds, reptiles and amphibia.

The National Academy of Sciences has elected to membership Professors John C. Branner, of Stanford University; William H. Holmes, of the Bureau of American Ethnology; William H. Howell, of Johns Hopkins University; Arthur A. Noyes, of the Massachusetts Institute of Technology, and Michael I. Pupin, of Columbia University. M. Henri Becquerel, of Paris, and Professor Paul von Groth, of Munich, have been elected foreign associates.

Professor E. B. Frost has been appointed director of the Yerkes Observatory by the trustees of the University of Chicago, in succession to Professor G. E. Hale, who gives his whole time to the establishment of the new Solar

Observatory of the Carnegie Institution at Mt. Wilson, Cal.—Professor John F. Jameson, head of the department of history at the University of Chicago, has been offered the post of director of the Bureau of Historical Research in the Carnegie Institution, Washington, D. C. This position is vacant through the return of Professor J. Lawrence Laughlin to the University of Michigan.

LORD RAYLEIGH is about to retire from the professorship of natural philosophy at the Royal Institution, which he has held for eighteen years. He will be made honorary professor. Lord Rayleigh has given twenty-three Friday evening discourses and twenty-one courses of afternoon lectures at the institution.—Sir Patrick Manson has been invited to give the Lane lectures at the Cooper Medical College, California, this year. He will lecture on some aspect of tropical diseases.—Professor Hugo Münsterberg, of Harvard University, has declined the offer of a chair of philosophy, tendered to him by the University of Königsberg.

DR. E. F. NICHOLS, professor of physics at Columbia University, has been awarded the Ernest Kempton Adams research fellowship, recently established at Columbia University by Mr. E. D. Adams in memory of his son. Professor Nichols has at present leave of absence and is working at Cambridge University.—Dr. Nettie Maria Stevens, of San Jose, California, associate in experimental morphology at Bryn Mawr College, has been awarded the prize of $1,000 offered every two years by the Association for Maintaining the American Woman's Table at the Zoological Station at Naples and for Promoting Scientific Research by Women.—The Smithsonian Institution has made a grant of $250 from the Hodgkins Fund to Professor W. P. Bradley, of Wesleyan University for an experimental study of the flow of air at high pressure through a nozzle.—The following appropriations have recently been made from the Rumford Fund of the American Academy of Arts and Sciences: To Professor Charles B. Thwing, of Syracuse University, $150 in aid of his research on the thermo-electromotive force of metals and alloys; to Dr. Harry W. Morse, of Harvard University, $500 in aid of his research on fluorescence.

DR. C. J. MARTIN, director of the Lister Institute, London, has been sent to India to investigate the plague. It is understood that with several bacteriologists he will carry on work at Kasauli. The deaths from the plague in India average more than 30,000 a week in spite of all efforts which have been made to check its ravages.

PLANS have been filed for a fifteen-story building to cost $975,000, which Mr. Andrew Carnegie is to present to the Associated Societies of Engineers of New York. It is to be erected on the large plot from 25 to 33 West Thirty-ninth Street, and immediately adjoining it in the rear, facing at 32 and 34 West Fortieth Street, will be a thirteen-story club-house, which is to cost an additional $375,000, also part of Mr. Carnegie's gift.